

# Psychological Review

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The *Psychological Review* is devoted primarily to articles in the field of general and theoretical psychology. This area is obviously difficult to define and delimit, but in view of the large number of manuscripts sent to the editor on all kinds of topics an attempt has to be made to draw the line somewhere.

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## THE PSYCHOLOGICAL REVIEW

## PURPOSE AND LEARNING THEORY

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We shall be concerned in this paper with the notion of "purpose," a notion that has long troubled social and biological scientists. It has been held by some psychologists (4) that a phenomenological observation of organismic behavior immediately reveals the essential goal directedness, the purposiveness, of this behavior, and that to talk of an animal's purpose in reaching a goal is legitimate as long as "purpose" is "defined" behaviorally. Other psychologists (2) have insisted vehemently that teleological concepts such as "purpose" either have no place in an objective account of behavior, or at most can be introduced only after they have been derived from primary principles. In this paper we shall attempt to show how such terms as "purpose" and "teleology" may be used with scientific respectability in connection with the primary principles of learning. In fact, we believe that these notions are essential to psychology, and, when properly used, make clear just what it is that the learning theorist is making laws about. It should be emphasized at the outset that no attempt is being made here to reintroduce into psychology mental-

istic notions or entelechies that psychologists have labored so long to eliminate from their science.

We are aware that many persons, especially hard-headed experimentalists, wish to avoid any use of teleological concepts. This avoidance is understandable. Teleological concepts have been pre-empted in the past by those who have had little or no concern with the formulation of testable theory. Historically, "teleology" has been defined as the opposite of "mechanism." A teleological theory was taken to be one which explained the past in terms of the future. Purposes, ends, goals, or, in short, the terminal stage of any sequence of behavior acted in some unanalyzable way as one of the antecedent conditions necessary to reach the terminus. This usage of "teleology" has not proved useful in solving scientific problems.

It might be argued by some that if by "purpose" is not meant some metaphysical notion, the word should not be used at all, for it has too many mentalistic connotations. We feel justified in retaining the word, however, because this paper is concerned with goal behavior and with means and ends. But we are not going to talk about any animal as "having" a purpose any more than the engineers who work with

<sup>1</sup> The authors wish to thank Dr. David L. Olmsted and Dr. Richard Rudner for their careful reading of the manuscript and their helpful suggestions.

guided missiles that "seek" a target introject into the missile an entelechy or purpose.

We hope, in this paper, to redefine "purpose" and "teleology" in order to make them amenable to scientific psychological usage. We also hope to show the importance of these terms to learning theory and to show that the reinforcement theorists who have insisted most strongly that teleological concepts have no place in objective learning theory have made, paradoxically enough, essential use of the teleological frame of reference.

A significant methodological development in recent years has been a series of reformulations of the concept "teleology." Certain philosophers of science, principally Singer (3), Churchman and Ackoff (1), and also a number of scientists, prominent among whom are Wiener (6), and Rosenblueth and Bigelow (5), have been active in this effort. Their work has been extremely helpful in enabling us to make an analysis of the relationship between learning theory and teleological concepts. Our point of view is closest to that of Churchman and Ackoff. In the short space available it will be impossible to give a full account of their views or to show in just what way their views differ from ours.

#### TWO FRAMES OF REFERENCE

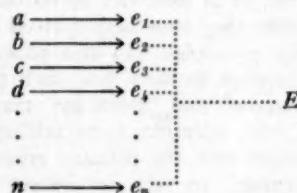
There are at least two quite different, yet compatible, frames of reference that may be profitably employed in the analysis of behavior. They are the mechanical and the teleological (but not in the historical sense previously noted). When a scientist uses the mechanical frame of reference, ideally at least, he attempts to specify the conditions, both necessary and sufficient, to bring about some state of affairs. For purposes of illustration, let us use the

following expression:

$$a \longrightarrow e_1$$

If the set of conditions designated by " $a$ " is sufficient to bring about the state of affairs designated by " $e_1$ ," then whenever  $a$  occurs,  $e_1$  occurs. If  $a$  is a necessary condition for  $e_1$ , then  $e_1$  will not occur unless  $a$  has occurred. As a matter of fact, scientists have seldom, if ever, been able to specify the sufficient conditions for any occurrence. Nonetheless, from the point of view of the mechanical frame of reference, the ideal to be achieved is the specification of the necessary and sufficient conditions for any event.

In the following diagram, let us assume that  $a$  is a necessary and sufficient condition for  $e_1$ ,  $b$  for  $e_2$ ,  $c$  for  $e_3$ , and  $d$  for  $e_4$ . This means that when  $a$  occurs,  $e_1$  will always occur, and if  $a$  does not occur,  $e_1$  will not occur. An explanation using the mechanical frame of reference may be said to have been given when a set of well-confirmed laws has been formulated which will enable one to predict the occurrence or non-occurrence of an  $e_1$  given the occurrence or nonoccurrence of an  $a$ .



There is another feature of the mechanical frame of reference that requires comment. It is essential to specify just what the antecedent and consequent conditions are in any sequence of events. Two ways of specifying objects and environments that have been distinguished especially concern us here. One is the physical classification and the other is the morpho-

logical. According to Churchman and Ackoff,

In physical classification we group objects or events (behavior patterns) in terms of a quantified property expressed along a physical scale, such as temperature, weight, wave length, velocity, etc. Morphological classification, on the other hand, is a non-physical method of classifying. It may take either a quantitative or qualitative form. When quantitative, the scientist chooses a range along a physical scale as a criterion for membership in the class. . . . We may also have qualitative morphologies, as when we specify "red objects," or "heavy objects," or "rapid walking" (1, p. 35).

The difference between physical and morphological classification is not that between precision and vagueness, but is the difference between the specification of a unique value along a scale and the specification of a range along a scale. The limits of any given range may be very precisely stated.

The ideal use of the mechanical frame of reference requires at least the physical classification of antecedent and consequent conditions. Otherwise it would be extremely difficult to specify the necessary and sufficient conditions for some occurrence. In our illustration, let us for the moment assume that *a* does not occur and *b* does. Under these conditions *e<sub>2</sub>* would appear. But if *e<sub>1</sub>* and *e<sub>2</sub>* are not distinguished on the basis of a physical classification, then *e<sub>1</sub>* might be confused with *e<sub>2</sub>* and *a* would seemingly fail as a necessary condition for *e<sub>1</sub>*.

*Means-ends.* The teleological frame of reference, as developed in this paper, is concerned with the notion of means-ends. Therefore the problem to which we now turn our attention is that of explicating this notion. We want to be able to answer the following question: Under what conditions may some specified occurrence be considered as a means to some end? In terms of our

illustration, we shall want to be able to answer the question as to when *a*, *b*, *c*, or *d* may be treated as means to an end. Let us begin by considering *e<sub>1</sub>*, *e<sub>2</sub>*, *e<sub>3</sub>*, and *e<sub>4</sub>* as falling within the same morphologically defined class, the range of which is at least extensive enough to include these four elements. Let us assume further that if at least one—and it makes no difference which one or ones—of these possible occurrences obtains, then *E* obtains. "*E*" is the name of the state of affairs that obtains when at least one of the four events (*e<sub>1</sub>*, *e<sub>2</sub>*, *e<sub>3</sub>*, *e<sub>4</sub>*) occurs. More precisely, "*E*" is a variable which takes names of states as instances of substitution. We require a name for such states of affairs in order to avoid awkward circumlocution. The question now arises as to the relationship between *a*, for instance, and *E*. It can be seen that *a* is not a necessary condition for *E* because even though *a* does not occur, *E* may occur through the occurrence of *b*, *c*, or *d*. However, *a* is the sufficient condition for *E*. That is to say, if *a* occurs, *e<sub>1</sub>* occurs and thus *E* obtains. What this analysis of the teleological frame of reference provides is an explication of the notion of means, and alternative means, to some specified end. The relationship of *a* to *E* is that of *a* being a means to *E*. Of course *b*, *c*, and *d* are also means to *E*, that is, means to the same end. The four alternatives can be described as potential means.

In comparing the two frames of reference, there are several important differences to be noted. First, in using the teleological frame of reference, it is permissible, although not necessary, to make a morphological classification of consequent conditions. It is not essential to be able to differentiate between, for instance, *e<sub>1</sub>* and *e<sub>3</sub>* since no matter which one occurs, *E* obtains. The successful employment of the me-

chanical frame of reference requires that both the antecedent and consequent conditions be classified at least physically. Second, in the mechanical frame of reference, the antecedent state of affairs is both a necessary and sufficient condition for the consequent state. In the teleological frame of reference, any given antecedent state is not a necessary condition for the achievement of the consequent condition, namely, *E* in our example, although it is a sufficient condition.

*Functional class.* At this point it is necessary to make one further distinction. In order to do this we introduce the notion of a functional class. Since *a*, *b*, *c*, or *d* have been treated as means or potential means to *E*, they can be thought of collectively as members of the same class. The criterion for membership in this class is that of being a potential means to some specified end. Thus, *a*, *b*, *c*, or *d* may differ greatly physically and/or morphologically and yet have the property of being means to some specified end, and thus collectively constitute a functional class.

#### REFORMULATIONS OF "TELEOLOGY"

Our object, in setting forth some of the essential features of the teleological frame of reference, is to enable us to make clear in just what way learning theory of the reinforcement variety *does* implicitly make use of this frame of reference. Reinforcement theorists seek to avoid any use of teleological concepts. In fairness to their position, it should be pointed out that they are opponents of that doctrine of teleology which leads directly to vitalism and emergentism, and which involves the introjection of an entelechy or purpose into the organism itself. Hull (2, p. 27) has suggested a way to avoid such subjective concepts. "This is to regard, from time to time, the behaving organism as a completely self-maintain-

ing robot, constructed of materials as unlike ourselves as may be." He believes that the tendency on the part of the observer to impute an entelechy, soul, spirit, or demon into such a robot is less likely than if the scientist were observing a living organism. He also states that the robot concept is an effective prophylaxis against the tendency to reify a behavior function. "To reify a function is to give it a name and presently to consider that the name represents a thing, and finally to believe that the thing so named somehow *explains* the performance of the function" (2, p. 28).

We, of course, are no more anxious than Hull to reify functions or to project entelechies into men or robots. It would seem, however, that the scientists who have been most intimately connected with the theory and construction of servomechanisms and sequence-controlled calculators have found it profitable to make extensive use of teleological concepts. Perhaps it would be more accurate to say that they have divested these concepts of their demoniacal connotations and rendered them usable for scientific purposes. Wiener (6) would characterize Hull's self-maintaining robot as active, purposive, teleological, and as capable of high-order predictions. One might assume that Wiener has simply repeated the ancient fallacy of projecting purpose into the machine itself. Quite to the contrary, his analysis—behavioristic in character—concerns only the input and output of the machine. He says, "Given any object, relatively abstracted from its surroundings for study, the behavioristic approach consists in the examination of the output of the object and the relations of the output to input" (6, p. 1). To say that a machine is purposive in Wiener's sense is not to say that it has some mysterious entelechy within it. The actions of the

machine may be assessed with respect to their being means toward some end. In so far as these actions are thus describable they may be said to be purposive.

The cybernetic definitions of teleological concepts have been framed with reference to the problems of interpreting the behavior of certain classes of machines and neural systems. This has resulted in a conceptual model which is workable for the problems of cybernetics. However, the data of current learning theory requires certain modifications of these concepts. Churchman and Ackoff (1) have recently framed a more widely applicable set of definitions which we shall draw upon for our present purposes. Wiener develops the idea that an object behaves purposively only if it pursues the same goal by modifying its behavior under varying conditions. The actions of a thermostat, for example, may be viewed as purposive in that the thermostat turns on the furnace when the room temperature drops below a critical point, and when the temperature goes above this critical point, it turns off the furnace. The end or goal in this example is the maintaining of the temperature at a critical point, and the actions of the thermostat may be viewed as a means of achieving that end in a changing environment. Let us, however, take for an example a naïve hungry rat placed in a Skinner box. This animal may perform a variety of responses and may, after a time, obtain food. But because the conditions in the box have remained constant, we cannot consider the rat's behavior purposive under the Wiener definition. Thus we are in the uncomfortable position of having to maintain that behavior of the thermostat is purposive, but the behavior of the rat is not.

To avoid this difficulty, we have worked out (following Churchman and

Ackoff [1]) certain teleological categories: (a) Extensive Function, (b) Intensive Function, and (c) Purposive Function. When using the teleological frame of reference one may classify any behavior as belonging to one of these categories. (a) If  $X$  (any object) accomplishes some specified end by displaying relatively invariant behavior in a wide range of environments, then  $X$  may be said to have an extensive function. (b) If  $X$  accomplishes some specified end by changing its behavior if and only if the environment changes, but exhibits only one type of behavior in any given environment, then  $X$  may be said to have an intensive function. (c) If  $X$  accomplishes some specified end by exhibiting different types of behavior, whether the environment changes or remains the same, then the actions of  $X$  may be said to be purposive. According to this schema, the behavior of the thermostat would be classified as having an intensive function; the behavior of the rat in the problem box as purposive.

#### HULLIAN THEORY AND "PURPOSE"

Let us turn our attention now more particularly to the Hullian formulation of learning theory. We wish to show that this theory makes essential use of the concept of purpose as just explained. In order to make our case, we must be very clear about *essential use*. To be precise about this extremely important point we must make the distinction between a set of lawlike statements and what they are about. Any theoretician has at least a twofold task: (a) He must classify the objects, events, or situations which are to be the subject matter of his laws, and this implies that he have a principle of classification. (b) He must formulate a set of lawlike statements, which have as their subject matter the objects, events, or situations that he has classified, and these

statements should systematically relate parts of the classified subject matter to other parts. Ideally, these lawlike statements will enable him to predict the occurrence of the classified happenings, and thus will acquire the status of laws. The learning theorist can properly be said to make essential use of teleological concepts if these concepts are employed either in his classificatory system or in his laws.

Hullian theorists have devoted their attention almost exclusively to the formulation of testable laws. But we have already noted that the theoretician has a twofold task and that he must do more than formulate laws. He must also classify the objects, events, or situations which will be the subject matter of his laws, and he must make clear the principle of this classification. It is this second task that the Hullians have neglected. Apparently they are unaware of what principle of classification they have used, and it is partly because they have neglected making clear this principle that the controversy about "teleology" exists.

Our point is that the Hullians have been using a classificatory principle, and that it is a teleological one of the purposive variety. Now that we have made this assertion, the question arises as to how it can be proved or disproved; what sort of evidence would be required to confirm or disconfirm it? Quite obviously we should turn to the actual work of the learning theorist. Let us take Hull's book *Principles of Behavior* (2) for illustration. We find that Hull, on the one hand, presents a number of experiments, and, on the other, he formulates a set of laws to explain what happens in these experiments. The question is: On what basis does he select these experiments as subject matter for his laws? We submit that each and every one of the experiments cited

in his book involves a situation in which the behavior of the organism is assessed with respect to its efficacy in accomplishing certain ends.

We find, for example, that Demonstration Experiment A on page 70 is concerned with a rat placed on a grid which is subsequently electrified. Hull points out that the rat displays a variety of responses, one of which, the leaping over a barrier, removes it from the electrified grid. Hull classifies the responses of the rat into two categories, the futile and nonfutile. What can Hull possibly mean by characterizing a response as futile? It is not a physical or morphological property of a response to be futile or nonfutile, and the notion of futility or nonfutility is certainly not part of the mechanical frame of reference. We believe that what he means, or should mean, is that certain of the responses of the rat, when placed on an electrified grid, are means to a specified end, and other responses are not. The end which Hull selects is need reduction. The rat's leaping over a barrier is a means to the end, need reduction. There is a further employment of the teleological frame of reference by Hull in this experiment. In a series of trials, the rat makes a number of escapes from the charged grid. Hull pays no attention to the exact location from which the rat leaps or to the specific musculature involved (for the rat may leap from varying stances) when he describes the rat's escape. If he were making a purely mechanical analysis, he would, of course, have to do this very precisely. Nor does he pay attention to the exact spot where the rat lands on the other side of the barrier, and this is something else that would have to be done precisely in a purely mechanical analysis. Let us assume that the rat may leap from any one of four physically different loca-

tions, and land on any of four physically different spots across the barrier.

Location  $a_1$  —————> Spot  $e_1$ ...  
 Location  $a_2$  —————> Spot  $e_2$ ...  
 Location  $a_3$  —————> Spot  $e_3$ ...  
 Location  $a_4$  —————> Spot  $e_4$ ...  
 Location  $E$

Hull is perfectly willing to count a landing at any one of these four spots as constituting an escape from the grid. What this means, then, is that he is interested in the relationship between  $a_1, a_2, a_3, a_4$ , which can be classified morphologically, and  $E$ . These four starting locations, and the appropriate jumping reactions, constitute members of the same functional class, any one of which is a means to the same end, namely, escape from the grid. The behavior of the rat on the electrified grid may be categorized as purposive. The rat accomplishes some specified end by exhibiting different types of behavior whether the environment changes or remains the same.

In "Demonstration Experiment C" (2, p. 75) Hull cites a conditioned-reflex learning experiment in which a dog learns to lift its foot in response to a buzzer, with shock serving as the unconditioned stimulus. Hull says, "No doubt the dog makes many other muscular contractions in addition to those which result in the lifting of the foot, but these are usually neglected in such experiments." Why, we might ask, are they neglected? It is quite evident that learning theorists are not merely interested in a purely mechanical analysis of what happens when an organism is subjected to certain stimuli. Rather, they wish to construct a set of laws that will enable them to predict the occurrence of those responses that are relevant to the achievement of a specified goal.

In the short space available we cannot cite all the experiments performed by reinforcement theorists, or even all

of those mentioned in Hull's book. We are confident, however, that an examination of the experiments performed by reinforcement theorists will reveal the same properties. What these experiments have in common is that they deal with objects that can exhibit different types of behavior whether the environment changes or remains the same. And the experimenter singles out of the myriad of responses that might be investigated just those that are relevant to the achievement of some specified end. What the experiments have in common, then, is the use of the teleological frame of reference, and even within that frame of reference, the use of the special category of purpose. For example, the experimenters do not treat behavior in terms of the categories of either extensive function or intensive function. The experiments are not performed on objects that display relatively invariant behavior in a wide range of environments, as do clocks, for example. Nor do they involve organisms or machines that exhibit different behaviors in different environments, but only one type of behavior in any given environment, as do amoeba and some servomechanisms. It should be clear at this point, then, that the learning theorists are making systematic use, as any theorist must, of some principle of classification in selecting subject matter about which to formulate laws. The Hullian learning theorists have never made their principle of classification clear.

#### TOLMANIAN THEORY AND "PURPOSE"

It may seem that the position that has been set forth in this paper is equivalent to the one Tolman and his followers have long urged. Certainly Tolman has talked about the importance of purpose, ends-means relationships, goals, and has even said that "behavior as behavior reeks of purpose

and of cognition" (4, p. 12). However, Tolman's use of teleological concepts is quite different from ours. To the Tolmanian, purpose is something that the animal has within it, and that an independent, neutral observer ascribes to the organism as a result of an analysis of its behavior. According to this view, we can speak, upon observing a cat in a problem box, of the "cat's purpose of getting to the outside, by bursting through the confinement of the box," or of the "cat's purpose as a determinant of the cat's behavior" (4, p. 13). To Tolman "The rat accepts or rejects and persists to or from food, blind-alleys, true path sections, electric grills, etc., only in so far as these latter function for him as subordinate goals . . ." (4, p. 28). In other words, goals are goals *to* the rat, and purposes are purposes *of* the rat. In our analysis, *purpose* is a function of the classificatory system employed by the investigating scientist; it is never some faculty inside the organism.

The Tolmanian analysis differs from our own in another important respect. In his system the distinction between the classificatory schema employed to select the subject matter for the laws and the laws of the system themselves is obscured. It seems to us that Tolman uses the word "purpose" both as the name of a phenomenon to be explained and as an explanatory device. This in itself is a procedure of dubious scientific value. Ultimately it is a viciously circular procedure. Moreover, it makes it extremely unlikely that a scientist can successfully carry out the twofold task that we have delineated above.

The learning theory developed by the reinforcement theorists, in our opinion, is not guilty of this confusion. It is, however, beyond the scope of the present paper to examine the relation-

ship between the Hullian laws and the implicit Hullian classificatory system in detail.

In summary, many critics have charged that reinforcement theory is defective because it does not take purposive behavior into consideration, and proponents of reinforcement theory have admitted the absence of teleological elements and have taken this absence as an index of the scientific character of their theory. Following the lead of certain cyberneticians and philosophers of science, we have worked out a new formulation of the teleological frame of reference which avoids metaphysical entanglement, and have shown that the reinforcement theorists have implicitly been making use of this frame of reference. The Hullian laws are set up to explain the conditions under which an organism learns. From the implicit Hullian point of view, to say that an organism has learned something is to assert that there is a high probability that the organism will exhibit responses that are a means to the end need reduction. Thus we have seen that the concept of learning itself is one that can be meaningfully used only within the teleological frame of reference.

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## THE PROBLEM OF "WHAT IS LEARNED?"

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In a recent paper (2) Kendler discusses the problem of "what is learned?" and concludes that it is a pseudoproblem stemming from the error of reifying theoretical constructs. It seems to us that Kendler's interpretation of the formulation, "What is learned?" as being synonymous with the formulation, "Which is the best theoretical model for representing the learning process?" is not a very reasonable one. We think there is another more important and operationally meaningful interpretation.

We interpret the formulation "what has the organism (O) learned in the series of situations (S)?" as being synonymous with the formulation "what changes have occurred in O's behavior and learning in any situation, whether it belongs to S or not, as a function of O's being presented to S?"

If the situations belonging to S are (approximately) identical, as has usually been the case in learning experiments, then one cannot infer anything about what has been learned, because (a) in a constant situation there is no possibility of knowing which cues or aspects of the proximal stimulation have been reacted to and, what is more important, (b) one cannot know on which distal variables the learning has been focused. The situation is a special case of what Brunswik (1) has called "channeled mediation" and "artificial tying" of variables.

This type of experimental design has often resulted in a failure to recognize the distal focusing of behavior, and has given rise to the frequent, but never very consistent, efforts to maintain a proximal-peripheral classification of variables, involving the belief that what is learned are movements in response to proximal stimulation.

As the learning situations become more variable, one may infer more about what has been learned in these situations. As the series of situations becomes more representative of the organism's ecological universe, it becomes easier to infer what changes have occurred in the ecological universe and, consequently, to infer on which distal variables the learned behavior is focused.

The proximal-peripheral classification of variables and the resulting primitive concept of what is learned, i.e., something that may be directly inferred from a constant situation, have led to the pseudoconcept of transfer. The procedure of determining what is learned in a series of situations (S) and the procedure of determining transfer from S are identical. They consist of varying the situation and of recording what changes in behavior and new learning have taken place as a function of the learning in S. Therefore the concept of transfer becomes unnecessary. The problem of predicting transfer is the problem of predicting what will be learned.

In addition to being superfluous, the concept of transfer can be shown to be an artifact created by the traditional experimental design. If transfer is defined as effects of learning in one concrete situation or group of situations on behavior and on learning in any other concrete situation or group of situations, then every instance of learning outside the psychological laboratory is also an instance of transfer, because every observation of learning requires at least two situations and every two situations, except in extremely well-controlled experiments, have at least some differences. Thus, when there is no artificial tying of variables, the concepts of learn-

ing and of transfer cannot be distinguished.

Let us now consider the problem of predicting *if*, *how fast*, and *what* a given organism or group of organisms will learn in a given series of situations. In our opinion the relevant variables are all central, and because these variables are the result of earlier learning processes, we arrive at the conclusion that, in general, every prediction of learning must, explicitly or implicitly, be based on a diagnosis of relevant parts of what the organism has learned before. In the limiting case of the newly born, the determinants of the learning process are not the result of earlier learning processes, but are nevertheless central.

By the statement that all variables relevant to the learning process are central we mean that they are variables of (earlier formed) systems that have to be inferred from a complex set of observations and that they are consequently not external. There is considerable evidence in literature (partly discussed in [4]) indicating that no constant relationships between simple external measures and the learning process can be found, except within extremely narrow fields. The motivational and perceptual determinants of the learning process in a given external situation are a function of earlier learning processes and the influence of the time factor (distribution of practice, delay of reinforcement, etc.) likewise changes with learning. We believe our point of view to be related to that of Young (5), who likewise points out the inadequacy of physical (i.e., simple externally anchored) measures.

An example of how the learning process is clearly a function of dimensions of central structures would be the case of a group of Communists and a group of anti-Communists memorizing the content of a speech given by Stalin. Here the relevant central structure is the atti-

tude toward Communism. Another example may be taken from animal learning. Studies of "place learning" and of "reasoning" (Maier) indicate that the rat, at the age when it is employed in learning experiments, has already formed what Piaget (3) has called "spatial groupings." This means that, given the information that a rat possesses the kind of central structure called "spatial grouping" and with such and such values of the parameters of this structure, one may predict (given certain additional information) if, how fast, and what this rat will learn in a given spatial context. Indices of the existence of a "grouping" of spatial relations are the ability of coordinating separate experiences, making detours, reversing paths, inferring directions, etc.

We conclude that the problem of what is learned, as defined above, has a clear-cut operational meaning and that it is an important one in the psychology of learning, because any successful theory of learning presupposes (a) that methods of diagnosing central structures (what has been learned earlier) have been developed and (b) that the laws relating the learning process to the dimensions of these earlier formed structures have been found.

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## A SINGLE THEORY FOR REMINISCENCE, ACT REGRESSION, AND OTHER PHENOMENA<sup>1</sup>

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The theoretical formulation here presented was at first designed simply as an attempt to explain in terms of Hullian constructs the act regression phenomenon reported by Hamilton and Krechevsky (9), Kleemeier (21), O'Kelly (32) and others. The specific theoretical model used, in fact, is a modification of Spence's transposition theory. It soon became evident, however, that the model might become useful not only for the prediction of regression phenomena and transposition, but also for certain features of retroactive inhibition, spontaneous recovery, and disinhibition. If upheld by further research, the theoretical model gives signs of being a very useful tool in the unifying of several areas of behavior study.

It should be emphasized that the theoretical model here presented attempts to deal only with a limited segment of behavior. The model is not to be considered an attempt to predict all of behavior.

### THEORETICAL MODEL

The application of the stimulus generalization principle to the two processes of excitation and inhibition as previously used in discrimination learn-

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<sup>2</sup> The opinions or conclusions contained in this paper are those of the author. They are not to be construed as reflecting the views or endorsement of the Department of the Air Force.

ing (39) provides a mechanism in terms of which predictions can be made in the areas of instrumental learning, verbal learning, and conditioning.

Eight assumptions will be used to indicate the nature of this mechanism. The specific functional relations required to state the assumptions precisely are not crucial in our present lack of knowledge of the interactions exhibited in the empirical results.

The eight assumptions involved in the model are:

1. Each time a particular response  $R_1$  to a given stimulus  $S_0$  is rewarded, a hypothetical excitatory tendency of  $S_0$  to elicit  $R_1$  is increased. The amount of this excitatory tendency connecting  $S_0$  to  $R_1$  will henceforth be referred to as the *Absolute Excitatory Strength* of  $S_0$  to elicit  $R_1$ .

This assumption is widely held by psychologists. McGeoch (28), in his chapter on the law of effect, summarizes the verbal learning data on this point and finds strong evidence in favor of a reinforcement position. Hull (15) summarizes the data on this topic in instrumental learning and conditioning, and he comes to the same conclusion in this area.

2. Each time a response  $R_1$  is elicited, either overtly or covertly, by a stimulus  $S_0$  and no reward occurs, a hypothetical inhibitory tendency of  $S_0$  not to elicit  $R_1$  is increased. The amount of this inhibitory tendency preventing the occurrence of  $R_1$  upon the appearance of  $S_0$  will henceforth be referred to as the *Absolute Inhibitory Strength* of  $S_0$  to  $R_1$ .

The experimental work relevant to this assumption has been limited in

amount. Melton (30, 31) suggests that in verbal learning some factor other than the competition between responses seems to be operating in those situations in which a second response ( $R_2$ ) is learned in place of the original response ( $R_1$ ); he calls this additional factor "Factor X" and considers it an unlearning factor due to the occurrence and nonreward of  $R_1$  to the stimulus when  $R_2$  is the correct response. Melton points out that if competition between the two responses is the only consideration leading to the "forgetting" of  $R_1$ , then the effect of  $R_1$  competing with  $R_2$  should be as great as the effect of  $R_2$  competing with  $R_1$ ; therefore the decrement to  $R_2$  due to proactive inhibition should be as great as the decrement to  $R_1$  due to retroactive inhibition. Since the retroactive-inhibition decrement was much greater than the proactive-inhibition decrement, Melton concluded that both an unlearning factor and a competition factor contribute to the decrement of  $R_1$  upon the learning of  $R_2$ . The fact that few overt intrusions of  $R_1$  occur during the learning of  $R_2$  can be made consistent with a sizable decrement in  $R_1$  due to unlearning. Melton suggests, by assuming that  $R_1$  responses occur covertly during the learning of  $R_2$ .

Operationally, extinction in both instrumental learning and in conditioning can be defined as a function of nonreinforcement; consequently, it appears safe to state that nonreinforcement produces a weakening of the S-R connection in both these areas.

3. The absolute excitatory strength of  $S_0$  to elicit  $R_1$  and the absolute inhibitory strength of  $S_0$  upon  $R_1$  summate algebraically. The algebraic difference between the absolute excitatory and inhibitory strengths of an  $S_0$ - $R_1$  connection will henceforth be referred to as the *E-I Strength* of  $S_0$  to elicit  $R_1$ .

4. When two incompatible responses,  $R_1$  and  $R_2$ , compete for elicitation, the difference in *E-I* strengths (determined algebraically) of the two tendencies is effective in determining the response made. The strength of  $R_1$  as a function of the difference between the *E-I* strength of  $R_1$  and  $R_2$  will henceforth be designated the *Effective Strength* of  $R_1$ .

Evidence for assumption 4 is suggested by the results reported by Melton and Irwin (30), McGeoch (26), and Siipola and Israel (37). All of these investigators suggest that increasing the number of presentations of  $R_2$  decreases the strength of  $R_1$ , though the results of Melton and McGeoch seem to suggest that this relationship is not necessarily monotonic when extreme overlearning of  $R_2$  occurs.

Assumptions 3 and 4 both involve statements of linear relationships between excitatory and inhibitory strength. Probably the relationships are more complex than simple linear functions. However, linearity seemed the best possible guess at this stage of knowledge. Spence (38) reports the assumption of algebraic summation of excitatory and inhibitory strength to be satisfactory as a first approximation.

5. Stimuli other than  $S_0$  acquire a tendency to elicit and to inhibit the response  $R_1$  as a result of the generalization of the hypothetical absolute inhibitory and absolute excitatory tendencies along several stimulus similarity dimensions. The curves will be termed the *Generalization Gradients* for inhibition and excitation.

Evidence supporting the concept of generalization of absolute excitatory strength in verbal learning has been provided by Yum (44), Dulsky (3) and others.

Lashley and Wade (24) have recently attacked the existence of a generalization gradient of excitatory strength in instrumental learning. However, replies by Hull (16), Grice

(4, 5), Grice and Saltz (6), and others to the Lashley and Wade paper appear to make the assumption of such a gradient a reasonable one.

Generalization of absolute inhibitory strength has not been directly investigated with verbal learning materials; when nonverbal stimuli are used, however, there seems to be little doubt as to the occurrence of the phenomenon. Hull (16) summarizes these data.

The following three assumptions are not implicitly or explicitly stated in Spence's transposition theory.

6. The slope of the curve for generalization of inhibition is steeper than the slope of the curve of excitation.

Assumption 6 concerning the relative slopes of the gradients of generalized absolute excitation and inhibition has few experimental data to back it. Hovland (13) reports that in conditioning there appears to be a tendency for inhibition to drop off more quickly than excitation as degree of generalization is increased.<sup>3</sup> However, there appear to be no data relevant to this assumption in the area of verbal learning.

7. After the removal of a stimulus, its trace persists for a brief period; the trace grows progressively weaker with the passage of time.

The above is obviously a restatement of the stimulus-trace hypothesis of Pavlov (33) and of Hull (15). The trace hypothesis was rephrased at this time to de-emphasize the neurological aspects of the formulations found in both Hull and Pavlov.

The eighth assumption is merely a statement of a specific condition of as-

<sup>3</sup> It should be noted that while Hovland's verbal conceptualization of inhibition is different from that proposed in this paper, the two inhibition concepts are isomorphic in terms of the operational definition offered in this paper and the operational definition implicit in the Hovland experiment.

sumption 5 concerning generalization of excitation.

8. Passage of time, after the cessation of an activity, is accompanied by an alteration of "set" stimuli within the subjects. The original set stimuli can be recovered by a warm-up period consisting of activity resembling the original activity.

The evidence for the above assumption arises largely from the work of Irion (17, 18). Irion found that memory loss between learning and relearning could be decreased if, just prior to relearning, colors were presented to the subjects in a fashion similar to the manner in which the verbal material was originally presented for learning. Irion interpreted these results as indicating that the delay between original learning and relearning was accompanied by a loss of "sets" acquired during original learning. This constitutes a stimulus alteration.

The fullest description of the more basic interactions between the above variables is presented in the following section on act regression. The discussion of the relationship of other experimental phenomena to the model will simply allude to the discussion presented in the act regression section.

#### INSTRUMENTAL LEARNING

##### 1. *Act Regression*

In the classical act-regression studies, one response (e.g., turning left in a T maze) is established to a particular stimulus and then is extinguished while a competing response (e.g., turning right in a T maze) is developed to the same stimulus. During the acquisition of the second response a new stimulus (e.g., a momentary electric shock) is presented at the choice point before the response has been made in that particular trial. Upon presentation of the new stimulus the animals often tend to exhibit the first (extinguished) response

more frequently than the more recently learned second response.

Hamilton and Krechevsky (9), Sanders (36), Martin (29), O'Kelly (32), and Kleemeier (21), among others, have used fundamentally the above experimental design. Both Martin and Kleemeier have shown that regression tendency increases with a greater relative number of reinforcements to the first response. O'Kelly has demonstrated that reducing the animal's drive level (satiating the animal) has the same effect as electric shock in producing regression.

In terms of the theoretical model, an absolute excitatory tendency,  $R_1$  (turning left), is developed to  $S_0$  (the choice-point stimulus) by means of repeated reinforcements. Next  $R_1$  is no longer

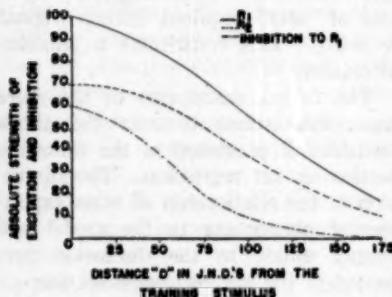


FIG. 1. Hypothetical generalization gradients of excitation and inhibition.

For illustrative purposes, the equation for generalized excitation was taken as  $E_j = E_0 e^{-0.00025d^2}$  and the equation for generalized inhibition was taken as  $I_j = I_0 e^{-0.00008d^2}$ .  $E_j$  and  $I_j$  represent excitatory and inhibitory strengths, respectively, at a generalized stimulus  $j$ ;  $d$  is the distance in *j.n.d.*'s between  $j$  and the training stimulus; and  $e$  is a constant equal to 10. These parameters are arbitrarily selected and are not intended even as approximations of the correct parameters. However, any parameters should show the same tendencies for interaction characteristics between responses as the above parameters, as long as both excitation and inhibition generalization gradients fall monotonically, and as long as the inhibition gradient falls more rapidly than the excitation gradient.

reinforced upon presentation of  $S_0$  and some incompatible response,  $R_2$  (turning right), is established instead. Repeated nonreinforcement of overt and covert  $R_1$  responses results in the development of inhibition to  $R_1$  at the training stimulus  $S_0$ , while repeated reinforcements of  $R_2$  lead to the growth of absolute excitatory strength to  $R_2$ . At the point where the *E-I* strength of  $R_1$  falls below that of  $R_2$ ,  $S_0$  will elicit  $R_2$  rather than  $R_1$ . The occurrence of any modification of  $S_0$  at this point will change the stimulus situation for the subjects so that some stimulus  $S_j$ , different from  $S_0$ , will be present. Figures 1 and 2 illustrate the effect of this state of affairs upon the effective strength of  $R_1$ .

As the stimulus changes in *j.n.d.*'s we find that the *E-I* strength of  $R_1$  to the generalized stimuli falls less quickly than does the *E-I* strength of  $R_2$ . This differential reduction in the *E-I* strengths follows from the consideration of the generalized strengths of excitation and inhibition associated with  $R_1$  and the strength of the excitation tendency to  $R_2$ . Since the slope of the inhibition gradient is such that inhibition falls more rapidly than excitation, the loss of absolute inhibition to  $R_1$  is greater than the loss of absolute excitation as the two move along the stimulus alteration dimension; therefore, the *E-I* strength of  $R_1$  will, up to some point, increase with stimulus alteration. As the inhibition to  $R_2$  is zero, the *E-I* strength of  $R_2$  will drop consistently as a function of stimulus alteration.

Figure 2 shows the effective strength of  $R_1$  at the various stimulus generalization points and shows how increased stimulus changes, at least up to some point, tend to allow for greater and greater effective strength of  $R_1$ ; this same tendency occurs whether the absolute strength of  $R_1$  is greater or less than that of  $R_2$ , but the stronger  $R_1$  is

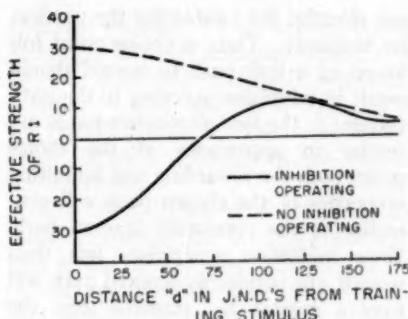


FIG. 2. Hypothetical generalization gradients of excitation affected by inhibition and not affected by inhibition when inhibition generalizes less than excitation. The values used to plot the above figure were obtained from Fig. 1.

in absolute strength in relation to  $R_2$  the greater will be the effective strength of  $R_1$ . Thus the two findings that have emerged from the regression studies are both predictable on the basis of the present theoretical model: (a) Shock at the choice point, altered hunger state, and altered set conditions lead to regression. (b) The greater the number of reinforcements of  $R_1$  over  $R_2$  during the original learning the greater the amount of regression.

## 2. Transposition

A classical example of transposition is one in which a subject first learns to respond to the larger of two stimuli; when the smaller of the two stimuli is removed and a new stimulus which is larger than the original large stimulus is introduced, the subject may respond to the larger of the new pair just as he responded to the larger of the old pair. In those cases where the subject reacts to the "relationship" between sizes, going to the larger stimulus despite the fact that this was not the stimulus originally rewarded, transposition is said to have occurred. Such "relationship" responses to new stimulus pairs, as can

be readily seen, are possible whenever the stimuli can be arranged along a dimension. Brightness has also been used quite often to demonstrate this effect.

The assumptions, in the theoretical model presented in this paper, which are not part of Spence's (39) transposition theory do not appear relevant to transposition. Consequently, the predictions originally made by Spence also follow from the present theory.

Spence hypothesizes that the reward member of the pair of stimuli generalizes excitation; the nonreward member generalizes inhibition. He then predicts from the interactions of these two processes that transposition is greatest to pairs of stimuli close, on the stimulus dimension, to the originally learned pair. Pairs progressively farther from the original stimuli should show the effect less strongly. Finally, pairs at the extreme of the dimension should show an inversion of the relationship response. Several experimental studies tend to sustain both the predictions of the inversion of the relationship response (8, 41, 43) and of the progressive decrease in tendency toward transposition as a function of distance from the original training stimuli (7, 22, 39).

However, the results of Kendler (20) are not completely consistent with the last stated prediction. Kendler measured the percentage of transposition along a brightness dimension. She trained one set of animals to respond to the brighter of two stimuli, then presented them with test pairs which were brighter than the training pair. The percentage of transposition first decreased in accordance with the prediction; however, without reaching a stage in which the percentage of transposition was that to be expected by chance, the curve began to rise again. These results are contrary to Spence's original prediction. However, they can be ex-

plained in part at least by the findings of Grice and Saltz (6); Grice and Saltz tested Hull's stimulus dynamism postulate and found that increased intensity of the stimulus resulted in increased response tendency. Kendler's discrepant results occurred when she presented stimuli brighter than the training pair. The relationship response was the one to the brighter stimulus. This stimulus dynamism postulate suggests that increasing brightness raises the response tendency. Thus the interpretation can be made that the tendency toward the relationship response was increased at the extremes of brightness used by Kendler.

Kuenne (23) has shown that the transposition follows the Spence predictions in preverbal children, but that in older children the relationship response appears to be stable and independent of the distance between the test pair of stimuli and the training stimuli. She interprets this as indicating that words take on specific cue value and that older children do not respond to the visual stimuli as such but rather that they respond to verbal cues like "bigger than" or "smaller than."

### 3. Spontaneous Alternation

Dennis (2) reports that when animals are given two alternative paths of equal length, both paths ending in reward, the animals will show a greater-than-chance tendency to avoid the path chosen on the preceding trial. The longer the paths are made, the stronger the alternation tendency becomes. This is a reasonable result, in terms of the theoretical model presented above. Delay of reward is a point along the reward-no-reward continuum. In fact, any response is eventually followed by some reward, though this reward may not arise until hours after the response has been made; in such a situation the reward may probably be considered as

not effective for reinforcing the particular response. Thus, a choice point followed by a long path to reward should result in inhibition accruing to the path chosen; if the two alternative paths are similar in appearance at the choice point, both the rewarding and inhibiting properties of the chosen path will generalize to the previously ignored path. Since inhibition generalizes less than reward, the previously ignored path will have a greater *E-I* strength than the previously selected path.

Passage of time has been assumed to alter the animal's set, and this results in generalization of both excitation and inhibition. Since inhibition generalizes less than excitation, it is predicted that the alternation tendency should decrease as a function of the time interval between the two trials. This is exactly what Heathers (10) found.

The more the two alleys were dissimilar in appearance at the choice point, the less generalization of either excitation or inhibition there should be between them. Consequently, the alternation tendency should be reduced. If the choice-point response is followed by immediate reward, no inhibition should develop and consequently alternation should occur no more frequently than would be expected by chance. The greater the amount of reward, the less alternation should occur as the *E-I* strength of the first alley will be increased. These three predictions, as far as the author knows, have never been tested. They would contribute considerable evidence concerning the theoretical model.

## VERBAL LEARNING

### 1. Retroactive Inhibition

As can be seen by examining the paradigm of act regression discussed above, the retroactive-inhibition paradigm and the act-regression paradigm are ex-

tremely similar. The act-regression technique can be described operationally as a modified retroactive-transfer paradigm (A-B, A-K, A'-B') in which the modification in the "relearning stage" involves a change in the stimulus conditions.

As a consequence of the theoretical model presented above, it is predicted that when, in the relearning phase of a retroactive-inhibition situation the stimulus member is altered within certain limits, the relearning should be faster than when the identical stimulus is presented.

*2. Strength of Retroactive vs. Proactive Inhibition as a Function of the Interval Between the Learning of A-K and the Relearning Phase*

Melton and Von Lackum (31) found that after learning A-B followed by A-K it was more difficult to relearn A-B than to relearn A-K. This, it will be recalled, was used by Melton and Von Lackum as proof for the existence of an "Inhibition" factor.

Underwood (42) repeated the Melton and Von Lackum experimental design using paired associates and using two different time intervals between the A-K and the relearning phase. Basically, one group relearned after 5 hours, another group after 48 hours delay. His results were the same as those of Melton and Von Lackum in the 5-hour delay group: The A-B list was more difficult to relearn than the A-K list. However, this was no longer true after 48 hours delay. The 48-hour delay group showed no significant difference in relearning for A-K over A-B.

The Underwood data become predictable in terms of the theoretical model if Irion's warm-up effect experiments are brought into the picture. If the interval between learning and relearning is accomplished by a stimulus change, the amount of change being a

function of the length of delay, then Underwood's 48-hour group would relearn A-K and A-B under a more altered stimulus complex than would his 5-hour group. As a consequence, it is predicted from the theoretical model that A-B would be stronger after 48 than after 5 hours. This, fundamentally, is what Underwood found.

*3. Bowed Serial-Learning Curve*

The typical curve of item difficulty in a serial-learning list of homogeneous items is bowed in appearance. An item somewhat closer to the end of the list than to the beginning is the most difficult to learn. Difficulty falls off in a monotonic fashion in both directions from the most difficult item. Consider A-B-C-D-E-F-G as constituting a serial list, each letter representing a separate item of the list. The correct response to any item of the list is the item immediately to its right. However, in addition to the association A-B, the remote forward associations A-C, A-D, etc. are formed, as has been shown by McGeoch (27), among others. And in addition to incorrect forward associations, McGeoch (27) and others have shown that backward associations develop; item D, for example, has a tendency to evoke items C, B, and A as responses. These remote associations may be considered as products of the stimulus trace. The trace of an item occurs simultaneously with some remote list item. When the remote item is reinforced to its correct stimulus, this reinforces the connection between the trace and the remote item. In the present paper, both correct responses after the response word appears and correct anticipations will be considered reinforcing states of affairs. The overt and covert occurrence of incorrect forward and backward associations is not rewarded, and increments of inhibition develop to prevent these responses from occurring to incorrect stimuli.

This inhibition generalizes along a similarity dimension so that the strength of the response is decreased not only to incorrect stimuli but also to correct stimuli.

If certain assumptions are made concerning the relative strengths of forward and backward remote associations, the frequency of occurrence of items as incorrect responses to other list items, when plotted against serial position of the responses, will produce a curve identical in shape to the typical serial-learning curve. To the extent that the above analysis is pertinent to the prediction of the bowed serial-learning curve, the items in the most difficult-to-learn serial position should be associated more strongly to incorrect stimuli than any other item. The item in the most easily learned serial position should be associated least often to incorrect stimuli. The present writer tested these predictions in a study to be reported in more detail at a later date, and the predictions were sustained.

#### 4. Reminiscence in Serial Learning

The passage of time produces an altered stimulus condition, with inhibition generalizing less than excitation. Therefore, following a rest after learning much of the inhibition will no longer be effective. As the inhibition is greatest in the central portion of the serial list, delay after learning will produce an increment in response strength of these center items. The loss of inhibition will produce an increased tendency toward false responses at the ends of the serial list; thus, more correct responses should occur at the center of the list, fewer at the ends of the list than before the rest period. These predictions correspond to the results obtained by Hovland (14).

Irion's (17) discussion of the warm-up effects indicates that, several trials after the end of the rest period, the Ss'

original set conditions are recovered. Consequently, the prediction can be made that after a warm-up of several trials the center of the serial-learning curve would again drop in number of correct responses; this should occur because the warm-up should return the Ss to their original set stimulus conditions where the inhibition effects are active. The Hovland data bear out this prediction also (14).

The prediction can be made that reminiscence effects will be due to recovery of words from the center of the serial list. Whether the *total* number of words recovered will be greater during the reminiscence trial than during the practice trial depends on the interaction between inhibition dissipation with passage of time and the interference effects caused by this dissipation. Generalization due to passage of time will dissipate the inhibition which develops in items due to specific non-reinforcements; it also dissipates the inhibition which accrues to items due to generalization of inhibition from similar items in the list. Since the exact gradients are unknown, it is difficult to make predictions about the optimal conditions in which total number of words during the reminiscence trials will be greater than the total number during the last practice trial. An interesting variation on the reminiscence studies has been attempted. This variation consists of presenting a meaningful passage for subjects to read. After the passage has been read, a series of questions on the passage is presented. Some of the questions quote the passage, others paraphrase it. Equating the two types of questions for difficulty immediately after reading it, Checov (1) finds that, after one day, the paraphrased questions are answered correctly more frequently than the direct quotation questions. From the standpoint of the theoretical model, the para-

phrased material is an alteration of the original material and produces the generalization necessary for reminiscence.

#### CONDITIONING

##### 1. *Disinhibition*

Pavlov (33) reports that an extinguished conditioned response often reappears when its conditioned stimulus is accompanied by an extraneous stimulus. This he calls disinhibition. In terms of the theoretical model, the extinction of a response is the non-rewarding of the response until the absolute inhibitory strength is approximately equal to the absolute excitatory strength. An extraneous stimulus alters the stimulus complex. As inhibition does not generalize as much as does excitation, the conditioned response will tend to reappear.

The prediction from the model is that as the extraneous stimulus alters the total complex more and more, the conditioned response should wax, then wane in strength. The waning results from an alteration of the complex to a point along the stimulus dimension where the absolute excitatory strength of the response is extremely small. These predictions coincide with the results reported in Pavlov (33).<sup>4</sup>

A more direct test of the model is possible at this point. If, instead of introducing an extraneous stimulus after extinction of the conditioned response, the conditioned stimulus is altered, disinhibition should result.

##### 2. *Spontaneous Recovery*

Pavlov (33) reports that an extinguished conditioned response reappears on the day after extinction. The reappearance of the response, as predictable from the model, is a result of the altered set stimulus which occurs

as a function of time. Since inhibition does not generalize as much as does excitation, the alteration of the set stimulus produces a generalization situation in which the excitatory strength of the response is greater than the inhibitory strength.

Also arising from the model is the prediction that learning a new response, since this involves a stimulus change, should facilitate spontaneous recovery. This, in essence, is the result reported by Liberman (25).

##### 3. *Inhibition of Delay*

When a long period consistently elapses between the onset of the conditioned response and the onset of the unconditioned stimulus, the conditioned response moves in time of appearance from immediately after the conditioned stimulus to immediately before the unconditioned stimulus.

In terms of the model, since there was a delay of reinforcement to the response when the response occurred immediately after the conditioned stimulus,<sup>5</sup> inhibition accrued to the response. However, when the response occurred after the conditioned stimulus plus a time delay, it was followed more quickly by reward (the unconditioned stimulus) and was fixated here.

The theoretical explanation provided above for spontaneous recovery is applicable here. On a subsequent day after training of the delayed response, the appearance of the conditioned stimulus should result in an immediate appearance of the conditioned response. Rodnick (34) found this to be the case.

<sup>4</sup> The similarity of this explanation of disinhibition to those of Pavlov (33) and of Hull (15) should be noted.

<sup>5</sup> The initial occurrence of the conditioned response immediately after the conditioned stimulus, in such delayed-reinforcement situations, is partly an artifact of training. The delayed conditioning is difficult to establish. Consequently, a shorter interval between conditioned and unconditioned stimuli is usually used at the beginning of training, and this interval is increased as training progresses.

The prerequisite variables for disinhibition as explained above are also present in this formulation of inhibition of delay. As a consequence, the prediction can be made that an external stimulus, accompanying the conditioned stimulus, should result in an immediate appearance of the conditioned response. Rodnick reports this phenomenon (35).

#### 4. *Massing of Practice*

In dealing with the effect of massing on conditioning, the discussion in this paper will be modeled on the preceding discussion of massing in verbal learning. The essentials of that thesis involve a response becoming attached to "incorrect" stimuli because traces of the incorrect stimuli are present while the response is being rewarded; the incorrect S-R connections are inhibited and this inhibition generalizes to the correct S-R connections; massing produces stronger traces of incorrect stimuli since the trace strength is a function of time after removal of the stimulus.

In a similar manner, massing in conditioning may be thought of as producing connections between the conditioned response and incorrect stimuli. The conditioned response *itself* can be considered as producing a trace stimulus which can be attached to the next occurrence of a conditioned response; lack of reinforcement (or delay of reinforcement) produces inhibition to this S-R connection. In terms of this explanation, the inhibition originally develops between the response trace (which acts as a stimulus) and the conditioned response; however, the inhibition moves back in the conditioning sequence and eventually occurs at the onset of the conditioned stimulus. This is because the response trace is a conditioned response occurring at the time of onset of the conditioned stimulus, and as such

it becomes a mediating stimulus for the inhibition.

As a consequence of the above discussion, it is predicted that, under massed conditions, multiple conditioned responses (e.g., two or more consecutive conditioned eye blinks following the conditioned stimulus) will increase in frequency as a function of number of trials until increments of inhibition with further trials extinguish them.

One question arises immediately concerning the trace-response connection discussed above: What reinforcing state of affairs occurs which allows such a connection to form? To be consistent with the reinforcement position taken earlier in this paper, it is necessary to indicate the existence of some reinforcement for the connection between response trace and succeeding response. The answer to this question is fairly obvious. In Hullian terms, the conditioned stimulus is a primary reinforcing agent. Since this follows the trace-response occurrence, it reinforces this occurrence.

Two factors are, as implicit in the discussion so far, involved in the inter-trial interval: reinforcement and inhibition. The longer the interval (i.e., the less the massing of practice) the more the inhibition to the trace-response connection. This is true because longer intervals mean longer delay of reinforcement since the  $S_c-S_u$  of the next trial constitutes reinforcement. For the same reason, the longer the interval, the less the reinforcement of the interfering trace-response connection. Extreme massing, then, should reduce the inhibition to the interfering response and might be expected actually to facilitate conditioning. Fundamentally, this is what Hilgard and Marquis (11) report that Calvin found. Nine trials per minute result in poorer learning than do three per minute. However,

18 trials per minute result in better learning than do nine per minute.<sup>6</sup>

Also predictable from the model are the results reported by Jones (19), who found that at the beginning of practice after an interval, conditioning performance is better than during the original massed training. Such time intervals, it will be recalled, have previously in this paper been assumed to produce a set stimulus change; stimulus change permits inhibited responses to recur.

#### 5. Switzer Effect

Switzer (40) reports that during the first few extinction trials following massed conditioning, an *increase* in rate of conditioned-response evocation occurs. This result is easily explainable in terms of the regression model. Extinction trials constitute a modification in the stimulus patterns and cause a generalization of the excitatory strength of the S-R connection. They also cause generalization of the inhibition arising from the trace-response connection being nonrewarded. Since inhibition generalizes less than excitation, an increment in *E-I* is to be expected.

Hovland (12) has examined the Switzer effect in several different sets of experimental conditions. Knowing the results obtained by Hovland it is possible to make specific assumptions about the shape of the curves of excitation and inhibition so that Hovland's results could be predicted. However, the regression model in the form stated above is not applicable to an explanation of Hovland's data. While it might be interesting to make specific statements concerning the variable parameters, the writer feels that this is outside the scope of the present paper.

<sup>6</sup> These results are of particular interest in that the prediction was made before the writer was aware of Calvin's experiment.

#### CONCLUDING REMARKS

The present paper has outlined a modification of Spence's transposition theory in an eight-assumption model. The model was then shown to be relatively consonant with past research in the areas of instrumental learning, verbal learning, and classical conditioning. The writer is aware that such a demonstration can show only the possibility that a theory may have merit. Only the actual use of the model for designing new experiments can tell us how much predictive value the model has. Several such predictions were attempted in the body of this paper.

The writer is also aware that, despite his Hullian bias, the model does not fit neatly into Hullian theory. For example, Hullians have used the concept of work inhibition to explain such phenomena as spontaneous recovery, reminiscence, and spontaneous alternation. The present writer has dealt with these phenomena without using the concept of work inhibition. The extent to which either position is "correct" in this conflict (and indeed the extent to which there is a conflict) must await further research before a decision can be made. In general, a serious scientist must recognize the unfortunate probability that no theory proposed in psychology for some time to come will be exempt from at least some modification. A slow, bit-by-bit alteration of previous ideas appears to be the way in which scientific models of respectable predictive power develop.

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## OBSERVATIONAL DEFINITIONS OF EMOTION

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In the theoretical discussion of the topic of perception, psychologists for generations have emphasized that the perceived object is a function of the observer's "set to perceive." Strangely, this same relationship is often overlooked when investigators come into disagreement in defining perceptual objects in other areas of psychological investigation. While it is generally realized that in any research one investigator can observe only a portion of the many potentially observable sequences which are occurring, it is too frequently assumed that because one observer reports on an event of a named category, then another investigator under similar objective circumstances can and will observe the same sequence of events (if his interest is directed toward that same category). If the second investigator does not verify the observations of his predecessor, all too frequently charges and countercharges are exchanged with regard to (a) the objective circumstances (experimental situation) or with regard to (b) the fallibility of the observer; rarely is there an attempt made to cross-check the (c) observational set of the two investigators.

In general, such clashes occur with least frequency in those areas in which precise description of experimental situation and apparatus narrows the range of set variability to almost zero, as in the study of sensation or of the conditioned reflex. On the other hand, such disagreements occur with regularity in those areas which are concerned with the study of human behavior in molar situations, that is, in those areas in which set variability permits a wide

range of differential selection of significant events out of the composite observed (emotion, motivation, personality, social adjustment, as examples).

The writer has been interested for some years in the controversies concerning the concept of emotion. In this area in particular, "set to perceive" is clearly responsible for much of the diversity of factual and theoretical observations. Even though P. T. Young in his *Emotion in Man and Animal* (24) gave one of the most inclusive definitions of this term which has yet been written, he creates confusion in that he strives to include in a single observational description three fundamentally different perceptual viewpoints. Likewise, D. O. Hebb in his article "Emotion in Man and Animal: An Analysis of the Intuitive Processes of Recognition" (11), gives a striking example of the manner in which different perceptual sets produce data of unlike utility in studying emotional behavior in the chimpanzee. In this article he reported that in the Yale Laboratory of Primate Biology in Florida a

. . . thoroughgoing attempt to avoid anthropomorphic description in the study of temperament was made over a two-year period. . . . A formal experiment was set up to provide records of the actual behavior of the adult chimpanzees, and from these records to get an objective statement of the differences from animal to animal. All that resulted was an almost endless series of specific acts in which no order or meaning could be found. On the other hand, by the use of frankly anthropomorphic concepts of emotion and attitude one could quickly and easily describe the peculiarities of the individual animals (11, p. 88).

This latter type of description, he asserted further, provided "an intelligible and practical guide to behavior" of the individual animals which was not found in the description of the separate acts.

Considerably earlier, in the contrasting summaries presented by Landis (16) and Bard (1) in Murchison's *The Foundations of Experimental Psychology* (1929), it was evident that differing perceptual sets were producing remarkably unlike data (yet not necessarily conflicting, and all quite properly classified under the same general topic of emotion). Later, this disparity of data led to the series of articles by Dashiell (6), Dunlap (10), Meyer (19), and Duffy (8, 9) with the common theme that the term *emotion* no longer had any proper use within the frame of scientific psychology.

Nevertheless, it is the contention of this paper that much of the seeming confusion and controversy concerning the concept of emotion would be removed if the differing perceptual sets of the contributors were recognized.

If the logic of the following schematic analysis is adopted, there are only three fundamentally different points of view which are available for use in the observation of emotional phenomena. However, the simplicity of this analysis is reduced when it is recognized that within the frame of each of these points of view there are possible significantly different perceptual sets and, within each of these sets, multiple levels of description (microscopic to macroscopic) may further complicate the observational report.

These three fundamental points of

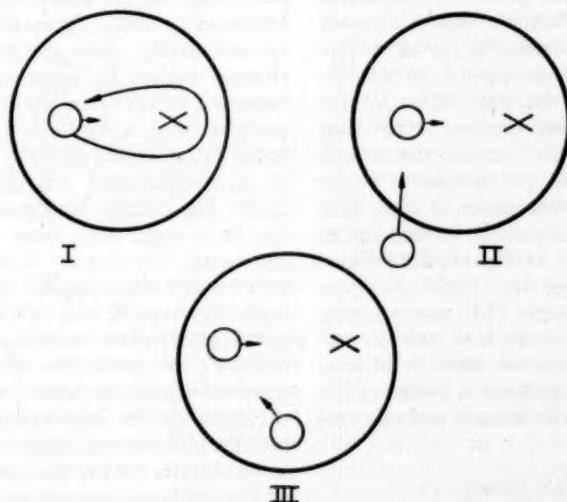


FIG. 1. Schema illustrating observational viewpoints in the study of emotion. In the above diagrams the large circle is intended to represent the extent of the effective stimulating environment impinging upon an emotional organism at any moment of observational study (obviously the cumulative effect of the stimulations of previous moments is not shown). The organism which is being observed is indicated by the small circle within the larger one. The dominant aspect of the immediate environment which is the focus of the organism's emotional attack or withdrawal is represented by an X. A second small circle with an arrow attached indicates both the reporting observer and the direction of his observation.

view are presented in the accompanying diagrams. They may be identified as: I. Introspection; II. Objective Observation; and III. Participant Observation. They are to be distinguished one from the other in terms of the relationship of the observer to that which is being observed.

#### INTROSPECTIVE OBSERVATION

In Diagram I is presented the point of view of the introspective observer. Here, the emotional organism and the observer are the same individual, and, as a result, this position is subject to all of the limitations that arise from any attempt to carry on two highly conscious activities at the same time. This introspective approach was the method used by the earlier writers who were criticized by William James (14) in his famous Chapter on Emotion and it is likewise the point of view which was used by William James himself. James differed from the earlier writers only in that his perceptual set was directed toward the observation of organic and sensory changes rather than toward the vaguer feelings and mental reactions of his predecessors. Lange (15) differed from James in that, with his medical background, he was set to observe changes of the capillary blood vessels as being the significant phenomena. Dewey's (7) contemporary alternate hypothesis indicated a perceptual set to observe emotion in relation to ongoing personal activities rather than in relation to internal physiological changes.

#### OBJECTIVE OBSERVATION

In Diagram II is presented the point of view of the objective observer who keeps himself outside of the stimulating environment affecting the emotional organism. The Gesell observation dome, or the more recent technique using the one-way visual screen to hide the ob-

server, typifies close-range objective observation of this nature. Telescopic viewing, examination of motion picture or other graphic time records, or the reviewing of protocol or diary accounts, permits a similar objectivity at a greater distance (either spatial or temporal).

Historically, it may be indicated that this objective position was the unwitting point of view taken by Sherrington (22) and Cannon (2) in their early research inspired by the James-Lange theory of emotion. Where James had appealed to introspective observation, Sherrington and Cannon resorted to operative experiments with animals. Consequently, their observations on emotion were of necessity objective, even though the frame of original reference had been introspective.

Regrettably, even this objective point of view permits significant variations in perceptual set in observing emotional sequences. Many investigators have focused chiefly upon the physiological changes evoked by experimental environments which were presumed to be uniform (that is, uniformity of the external situation was stressed, while little or no consideration was given to the varied life history backgrounds which the Ss brought with them to the experimental situation). Cannon's (3) later experiments typify this physiological perceptual set. Other investigators have focused more upon the uniformities and variations of facial and gestural expression when members of the same species were exposed to supposedly uniform emotional stimulations, as did Landis (16) in his classical study of human facial expressions. Yet others, perhaps not identified as psychologists, have investigated the temporal antecedents or causes in the life environment which have led to similar emotional consequences in certain life situations. Injury-accident prevention studies in factories, traffic accident rec-

ords and analyses in our city and state traffic bureaus, and the sickness-death probability studies by insurance companies all fall within the pattern of objectivity proposed here; and all have been directed toward the reduction of potentially emotional events in the life experience of many citizens.

An interesting outgrowth of an emphasis upon extreme objectivity in observing emotional phenomena was the denial by the behaviorist Weiss (23) of the significant existence of emotional phenomena, since the concepts of feeling and emotion had their reference origin in introspection.

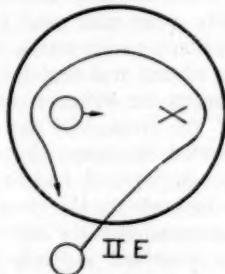


FIG. 2. Schema illustrating empathy, or projective identification. In the above diagram the observer is focusing his observations upon "how would I feel if I were in that situation."

Diagram IIIE represents the perceptual set of empathy or projective identification. This is the perspective of the observer who states, "I know just how the subject feels, for if I were in his shoes, I would feel thus-and-so." While this point of view is considered unscientific, it must be recognized in order that it may be guarded against by the observer who would be consistent in the position described in Diagram II.

Even though empathy is not a valid position for scientific observation, it is a viewpoint which must be understood in terms of its significance to most laymen. Much discussion of emotional

events by nonpsychologists (as well as by some psychologists) is carried on in the framework of this position. Likewise, this is the attitude which is most satisfactory when the scientist or the layman is attending a movie or play, or when he is reading stories or novels "for relaxation"!

#### PARTICIPANT OBSERVATION

Diagram III presents the third point of view: participant observation (13). In much of the research on emotion, this face-to-face observational position has not been differentiated satisfactorily from the two previously considered viewpoints. Yet, a brief study of the three diagrams should make it evident that this point of view, in terms of human interaction, is significantly different. Here the observer is a part of the stimulating environment of the emotional organism. In some instances, even, the observer and the X (object of emotional attitude) become identical for the emotional organism. The activities of the observer serve to enhance or reduce the intensity of the emotional behavior sequence in *S*. Likewise, there is the possibility that the observer himself may be provoked into emotional responses!

Psychologists are frequently approached by laymen requesting advice as to the means available for controlling the emotional behavior of others in such face-to-face situations. The layman as a parent may be concerned with the emotional outbursts of his children; as a husband he may be concerned with the emotional exchanges between himself and his wife; or as a teacher, policeman, or minister he may be seeking help in the person-to-person emotional encounters which are an inevitable part of the role he is playing in the social order. As a scientist, the psychologist has little professional information to offer in these situations,

although research on techniques of therapy (directive vs. nondirective, etc.) may provide tested information for the counselor in the near future. The psychologist as an artist in interpersonal relationships must draw upon his personal recollections, his common sense, his clinical experience, or upon his memory of similar situations as portrayed on the stage or in novels to offer constructive suggestions.

In psychological contexts other than those concerned with research on emotion some recognition has been given to this type of face-to-face observational situation. Moreno's (20) "role-playing" involves an approach of this nature without crystallizing the stimulative role of the observer. During World War II the OSS (25) project on personality assessment developed a test situation which is an embodiment of this point of view. The candidate, under an assumed identity, directed the activities of two young psychologists who were introduced to him as unskilled farm hands. The "farm-hand" assistants were instructed to misinterpret directions, to procrastinate, while at the same time engaging in needling conversation, all tending to throw the candidate off balance; throughout, these troublesome helpers as well as others were making note of the candidate's ability to cope with the frustrations and confusions of this unusual test situation. Such an assessment project differs from this third observational position only in that it was used for purposes other than the direct study of emotional behaviors.

Regrettably, many laboratory experiments which were set up with the intention of complete objectivity have nevertheless inadvertently slipped from the II position of objectivity to the III position of face-to-face observation. This slip may have been permitted because the experimenter con-

sidered it essential that he manipulate apparatus in coordination with *S*, or more simply that he be at close enough range to observe *S*'s responses. Some years ago the present writer published a report (18) of an experimental study of the effects upon "other hand" tension of electric shock punishment accompanying star tracing. Evidence was reported of considerable tension as measured by pressure upon a rubber bulb. A year later an advanced student was urged to collect more data on the same problem with the same equipment in the same location. The student found *little or no evidence* of tension. When the data of the unpublished study were compared in detail with the writer's earlier study, the only significant variant was that in the earlier experiment the writer as instructor used students from his own classes (with repeated assurance that participation, though required, had no bearing upon the *S*'s grade in the class), while in the later experiment the experimenter was easily identified as "only a more advanced student" who could have no influence upon the grade-giving instructor. Thus the tension reported in the published study was a function of the student-instructor relationship in this face-to-face experimental situation rather than of the electric shock.

Experiments upon behavioral phenomena other than the emotional are probably also susceptible to this face-to-face influence, particularly when prestige of the experimenter may be known to the *S*. A considerable number of check experiments would be in order to ascertain predictable distinctions between situations II and III.

#### LEVELS OF DESCRIPTION

In addition to the discrepancies which arise as a result of these differing observational viewpoints, there is, as men-

tioned earlier, a second basis of apparent divergence in the research studies on emotion which may be attributed to unlike levels of magnification or to unlike levels of description. Such differences in level may occur even when the fundamental point of view is identical from observer to observer.

Although the significance of the concept of levels of description is widely recognized in areas of practical measurement, it seems to have been frequently overlooked in the approach to psychological problems. Certainly it has been used rarely, if ever, in the structuring of interrelationships in the field of emotion.

A clarifying illustration of varying levels of description may be found in the English metric system for measuring length. Units are used, which vary from inches, to feet, to yards, to rods, to miles. Yet each of these units constitutes a level of description most appropriate to certain measurement activities. With small objects, such as pencils, envelopes, knives, etc., the inch is the convenient unit; with larger objects, such as lumber for building, automobiles, and furniture, the foot is the preferred unit; but still longer objects, such as a strip of cloth or a length of rope, will be dealt with in yards. Shorter distances in geographical space, such as the length of a field or a farm, are expressed in rods; while greater distances, such as those between town and town, or city and city, are expressed in miles.

A second example, that of money in economic exchange, reflects our practical acceptance of differing levels but illustrates how calibration is simplified by use of the decimal system. Our coins: cent, nickel, dime, quarter, and half dollar, are each appropriate to certain exchange activities, while our paper currency permits exchanges at various levels of higher descriptive value. Con-

ceptually, in affairs of government, the million and billion become the convenient descriptive unit. In this context it is appropriate to point out that a person habituated to values at one level may be incapable of adequate value judgments at another level, as expressed in the folk saying, "Penny wise, pound foolish." It is likely, though perhaps difficult to demonstrate, that such preferred levels of descriptive observation peculiar to the person may be present in other areas of measurement as, for instance, in the study of emotion.

Further citation of such examples could be given in many other areas of practical human manipulation of environment. One further significant relation, however, must be indicated before we return more specifically to emotion. While we have chosen to review this descriptive level concept with hierarchies in which the units of the various levels are now calibrated in terms of each other (i.e., twelve inches to the foot), this has not always been true. Irwin (12) in a recent article in the *Scientific Monthly* pointed out that the units of the English system of measurement arose out of independent manipulative activities in different segments of the population, and were not defined in reference to each other until more general usage made their calibration necessary.

Returning now to the conceptual framework of emotion, we find ourselves in a position in which the observer (in the objective or the participant points of view) is endeavoring to use immediately experienced behavioral cues, such as gesture, facial expression, tone of voice and language, as well as situational relationships as indicators of emotional attitudes and as predictors of probable direction of ensuing motivational behavior. In certain situations, as with money in economic exchange, there are probably available a

succession of appropriate levels of description. For brevity in illustration, only the two extremes of microscopic and macroscopic observation are presented here.

#### MICROSCOPIC LEVEL OF APPROACH

If investigating at this level, the observer directs his attention toward minutiae, such as the individual contractions of the 46 or more muscles of facial expression, as did Landis (16) in the analysis of one of his photographic research studies; or, the observer tries to record successive specific acts as did Hebb's (11) associates at the Yerkes laboratory. In either instance, the observer of emotional events usually finds himself in the position of the man who would measure the distance between cities with the inch as a unit; that is, the complexity of details make the measurements difficult to comprehend unless translated into a higher-level unit.

#### MACROSCOPIC LEVEL OF APPROACH

While the *Zeitgeist* of our scientific era has tended to make description on the macroscopic level appear to be common sense and therefore implicitly unscientific, still there is need for accurate predictive descriptions at this level, particularly for those who work with other humans in face-to-face relationships. It is essential that we anticipate and predict "the other fellow's next move" whether it be in poker, family argument, bargaining for a contract, or in phrasing a political agreement. Rarely does the "other fellow" submit to the use of a polygraph to help us ascertain his emotional uncertainties. Accordingly, most of us find it necessary to rely on the macroscopic concepts of emotion in such situations. Such concepts of emotion indicate varying degrees of uncertainty and disorganization which may be interpreted to our

advantage—although, conversely, these may carry the threat of explosive disruption if pushed too far. Hence, it behooves us to go further than just detecting the emotion: we must identify its nature and predict its course. Carr (4, p. 280), in his *Psychology*, summarized this macroscopic use of the concept: "Anger is correlated with an aggressive attack against obstacles, while fear is associated with the opposite type of behavior." Love, as a similar term, is predictive in a situational reference; that is, mother love implies emotional protection of offspring; sexual love suggests amorous approach toward the loved person; while brotherly love suggests support and defense of the sibling on other than rational grounds, and the like. Anxiety suggests disruption of most coordinated life habits to a noticeable degree; terror or panic obviously implies wild irrational behaviors in escaping some real or fancied threat. This catalog could be carried on at length, although with less agreement as to predictive implication when extended to synonyms and to more infrequently used terms.

Hebb's study (11), particularly in his finding that the frankly anthropomorphic concepts proved to have more predictable use, supports our contention that most of us in our living human face-to-face environments have developed higher-order concepts for use in identifying and in predicting emotional behaviors. However, an overview of the research on the interpretation of facial expression, hand gesture, and body posture in relation to emotion would seem to indicate that as in the earlier era of independent measures of length we have not yet succeeded in calibrating our units from higher levels to the lower ones. Application of the concepts of levels of magnification and of levels of description in this context should help greatly in reconciling many

conflicting interpretations of the research data published under the topic of emotion.

### SUMMARY

It is proposed in this paper that in the study of emotion, human perceptual limitations restrict the extent and the nature of observations which may be made at any time. It is suggested that this perceptual limitation extends as well to other molar concepts in psychology.

It is further proposed that much of the apparent conflict in this area could be reduced if the significant points of view were defined, and if the levels of description within each of these points of view were specified.

It is suggested that the most significantly different points of view may be specified as the (a) introspective, (b) objective, and (c) participant. The objective point of view occasionally carries the risk of being confused by the intrusion of projective identification. Of the three, the third (participant) has been differentiated with least clarity in the research literature on emotion.

It is indicated that several levels of description are available within each point of view, although only contrasting extremes are presented here. It is suggested further that we cannot adequately portray the whole picture until the various levels of measurement and description can be so calibrated that a given set of observations may be either integrated into higher units or differentiated into the units of a lower level.

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## DO INCORRECTLY PERCEIVED TACHISTOSCOPIC STIMULI CONVEY SOME INFORMATION?

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In a recent experiment on "subception," Lazarus and McCleary concluded that "a process of discrimination can operate prior to conscious recognition and in the absence of the possibility of the correct verbal report" (3, p. 116). The process with which they were concerned was autonomic discrimination among visual stimuli. They first established a conditioned GSR to 5 of 10 tachistoscopically presented nonsense syllables by pairing them with electric shock, and then measured the GSR for subsequent presentations of the 10 stimuli without shock. For only those stimuli which subjects failed to identify in a single verbal report, the average GSR following shock syllables was significantly greater than the average GSR following nonshock syllables. The gist of their results appears in Fig. 1.

Since the Lazarus-McCleary experiment was undertaken to provide support for a number of recent articles having to do with the influence of need on perception, their discussion of the problem is oriented in this direction. We have no quarrel with their procedures and data, but we regard as unfortunate the implication that they have discovered evidence for some sort of "unconscious determination of behavior" (3, p. 121) operating when "recognition . . . is impossible" (3, p. 114). We prefer to interpret these data as suggestive of a rather more obvious principle which is important for many psychological experiments: Even when an *S*'s first verbal response to a stimulus is wrong, the stimulus may still have conveyed useful information to him.

If this is so, it should be possible to demonstrate this transfer of information by some sort of verbal response in the absence of shocks, needs, or other strong emotional provocation.

### STATEMENT OF THE PROBLEM

The study reported here is an attempt to answer this question: Is it possible to show by some kind of verbal response that a stimulus has conveyed useful information, even when the stimulus has been incorrectly identified on the first trial? As our verbal response, we have used the number of additional guesses, following the initial wrong response, necessary to name the stimulus correctly. If an *S* needs fewer additional guesses than are to be expected by chance, we may infer that the likelihood of a correct response has been increased by the stimulus, i.e., that the stimulus has conveyed some information. A secondary aim of this study is to get some insight into the factors which operate to determine the *S*'s verbal behavior in a guessing task such as this.

### METHOD

#### *Subjects*

Ten male undergraduates served as *Ss*. None had had more than an elementary course in psychology.

#### *Apparatus*

The *S* sat seven feet from a milk-glass screen on which the stimuli were projected from a tachistoscope. A between-lens shutter and a variac in the bulb circuit of the tachistoscope permitted variations in both exposure speed and intensity. The stimulus words were five-letter double nonsense syl-

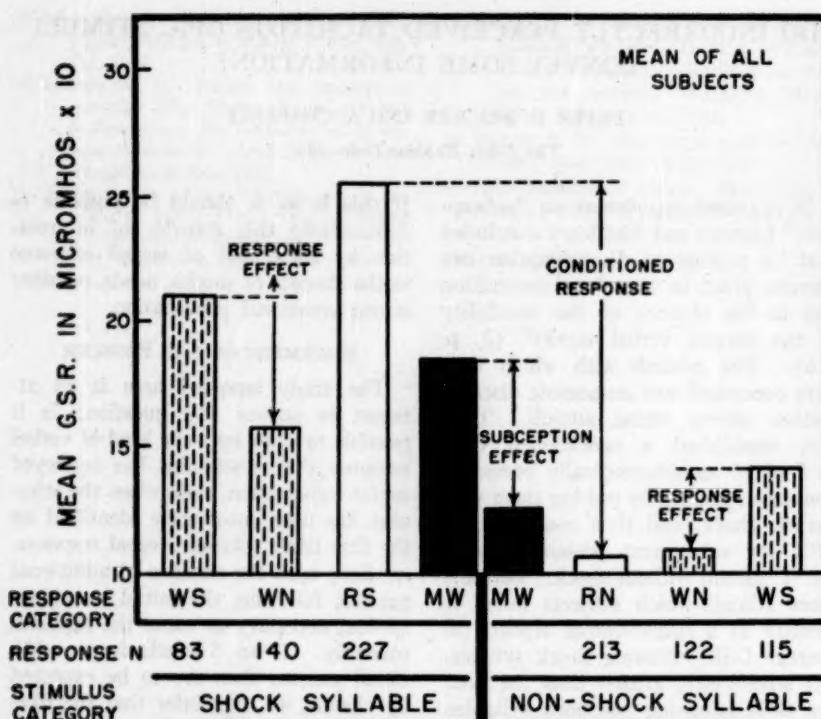


FIG. 1. Grouped data from the study by Lazarus and McCleary (3). The heights of the bars indicate the average size of the GSR over nine *Ss* for each of the classifications of stimulus and response. The Response Category labels may be interpreted with the following guide: W indicates that the response was wrong, R that it was right; S indicates that the response was one of the syllables which had previously been associated with shock, N that it was a nonshock syllable; and M denotes the arithmetic mean. Thus, the bars labeled MW are the mean GSR's for all wrong responses following each type of stimulus syllable. Each MW bar is also the average for the two striped bars in its stimulus category.

ables, or paralogs, printed on slides in capital letters. Eight of these stimuli (hereafter called the List stimuli) were listed on cards with which the *S* was supplied, and five of them (hereafter called the Nonlist stimuli) were known only to *E*. Both groups of stimuli are shown in Table 1. The *S* was provided with eight cards, on each of which was a different random arrangement of the same eight List stimuli.

#### Procedure

*Preliminary series.* The *S* was told that he was taking part in an experiment on

the training of perception, and was never informed of the Nonlist stimuli. He was also told to respond after each exposure, even if he had no idea what the stimulus was. During the preliminary series, the List stimuli were presented in random order, and *S* was allowed one guess for each stimulus. The *S* used his cards as guides in making his responses, putting the card he had just used on the bottom of the pack after each guess. This was a precautionary measure to prevent the *S*'s establishing order preferences in the use of the syllables. As the preliminary series progressed, *E* adjusted the shutter speed

TABLE 1  
LIST AND NONLIST STIMULUS WORDS\*

List Stimuli	Nonlist Stimuli
GOKEM	LAJYV
TAROP	NIGAT
LATUK	RUNIL
SIJUD	VECYD
HEZUW	YUZYJ
FEXAD	
MYZEG	
CEFIJ	

\* The List stimulus words are those the subject knew about and tried to guess. Subjects were not aware that the Nonlist stimulus words were also shown during the experimental trials.

and variac setting until *S* consistently got half or less of the stimuli correct. The speed and illumination arrived at in this way were used throughout the experimental run. This session also allowed *S* to become familiar with the stimuli and to achieve an asymptotic level of proficiency in recognizing them.

*Test series.* After a five-minute rest period, the experimental series of 120 presentations was begun. In this series, the 13 stimuli were arranged in random order, with the restriction that each List stimulus appeared ten times and each Nonlist stimulus eight times. One further restriction, which will be explained later, was that each List stimulus was designated as the correct response for each Nonlist stimulus only once. The *E* informed *S* that he must keep guessing after each stimulus, without seeing it again, until *E* said "right." The *S* again used his cards as guides, and changed cards only after completing a series of responses to one stimulus. The *S* wrote down each guess as he reported it verbally to *E* so that he would not repeat any guess which was wrong. The *E* said "wrong" or "right" after each guess.

Nonlist stimuli were included in the experimental series in order to obtain from each *S* a distribution of the number of additional guesses needed to identify the List words when they were not presented to him as stimuli. Since perceptual conditions were difficult, it was safe to assume, as later questioning of the *Ss* proved, that the *S* would not discover that stimuli other

than those on his cards were being presented. Hence when a Nonlist stimulus was presented, the *S* began making guesses from his group of eight List stimuli. For each presentation of a Nonlist stimulus, a List word was selected arbitrarily, within the limits described in the preceding paragraph, as the correct response. Thus all conditions which pertained when *S* was trying to identify a relevant stimulus were the same, except that the stimulus could not contain any useful information. This procedure is effectively the same as asking the *S* to make 40 series of guesses at the List word of which *E* is thinking, when *E* thinks of each of the eight words at random five times during the sequence of 40 trials.

## RESULTS

*List vs. Nonlist stimuli.* In answering our primary question, we worked only with those stimuli to which the initial response was wrong. The mean number of additional guesses necessary to make the correct response after (a) List stimuli and (b) Nonlist stimuli were computed for each *S* (see Fig. 2). The difference between these means was computed for each *S* and entered into a distribution of differences. The mean number of additional guesses for List stimuli for all *Ss* is shown in Fig. 2 as a solid line, and that for Nonlist stimuli is shown as a dotted line. Since the probability that the mean difference of .63 occurred by chance is less than .001, we conclude that significantly fewer additional guesses were needed to identify incorrectly-perceived stimuli than to guess names for stimuli when the names had no meaningful connection with the stimuli. In other words, the List stimuli conveyed some useful information to *S*, i.e., increased the likelihood of his making a correct verbal response.

To determine whether the guessing behavior following Nonlist stimuli was different from chance, we made use of all 40 guesses made by each *S* after the

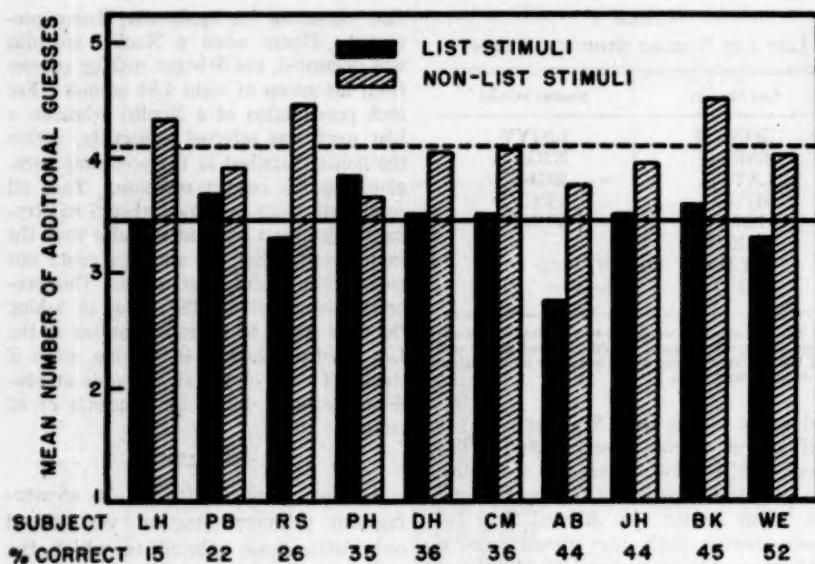


FIG. 2. Results of the test series for 10 Ss. The black bars show the mean number of additional guesses needed to identify List stimuli after an initial wrong response; the striped bars show the mean number of additional guesses needed to make the correct response after Nonlist stimuli. The solid horizontal line is the mean of the black bars; the dashed line the mean of the striped bars.

Nonlist stimuli. The mean number of guesses necessary to make the correct response following Nonlist stimuli was 4.58, while the mean to be expected by chance was 4.50.<sup>1</sup> The difference between the mean and the chance value is not significant, so we conclude that Ss were guessing at random when they had to identify List words following Nonlist stimuli.

The various Ss were able to identify from 15 per cent to 52 per cent of the List stimuli correctly on the first response, as shown in the bottom line of Fig. 2. This figure also shows that the magnitude of the difference between the two mean numbers of additional guesses

for each S is not related in any obvious way to the percentage of List stimuli which he was able to identify correctly on the first response.

*Word preferences.* The number of times each word was used as a first response was compared with expected frequencies for each S. This was done first for responses to List stimuli alone, and then for all 120 first responses. The combined chi squares for both of these conditions were significant beyond the .001 level, indicating that Ss respond more frequently with some words than with others. When the words are ranked for each S according to preference, however, the average intercorrelation by ranks among Ss is only 0.234.<sup>2</sup> This means that there was little con-

<sup>1</sup> The apparent discrepancy between this statement and the data of Fig. 2 is easily resolved by noting that Fig. 2 shows the mean number of *additional* guesses following initially wrong responses.

<sup>2</sup> The average rank intercorrelation was computed by the technique derived by Peters and Van Voorhis (5, pp. 200-201).

sistency among *Ss* as to which words were preferred or nonpreferred. However, the preference rankings of two of the words were quite consistent from subject to subject. LATUK was ranked first or second by 9 out of the 10 *Ss*, and third by the remaining one. SIJUD, on the other hand, was the least-preferred first response. For six *Ss* it ranked seventh or eighth, and its other ranks were 6, 5, 3, and 2. It is quite possible that LATUK occurred more frequently as a first response partly because it was more easily recognized than other words by most of the *Ss*.

*Word-sequence preferences.* Tabulations of first-order dependencies in sequences of two or more responses to one stimulus indicate preferences for certain sequences of words. Although our data are too few for many detailed comparisons, there are many instances in which words with similar elements (letters) follow each other. The data for one *S*, for example, show that the two permutations of SIJUD and FEXAD account for 44 per cent of the instances in which either is followed by another word. Another *S* said GOKEM more than half the time after MYZEG and HEZUW, while 80 per cent of the responses following GOKEM were one of the latter two words. Still another followed LATUK with TAROP 40 per cent of the time. CEFIJ and SIJUD seem to occur in pairs for many of the *Ss*. Some of the similarities which do not seem obvious when viewing conditions are normal might be clearer if we knew what elements of the words stand out under difficult tachistoscopic conditions.

*Word legibility.* Additional computations suggest that there are differences in ease of recognition (accuracy) among the words, and that the relative position of the words in this respect also varies from *S* to *S*. There were, how-

ever, too few responses per word to determine the relative accuracy of the words or to correlate word preference with accuracy within *Ss*.

#### DISCUSSION

Our results agree with the findings of Lazarus and McCleary (3) in providing an affirmative answer to the question posed in this paper: Even when *S* gives an incorrect verbal response to a stimulus, the stimulus may still have conveyed some useful information. Lazarus and McCleary demonstrated this by a conditioning technique. We have shown the same thing by using verbal responses. Following an initial wrong response, the number of additional guesses necessary to name the stimulus correctly is fewer than would be expected by chance.

Unlike Lazarus and McCleary, however, we look for more ordinary interpretations of our results. We believe that in any situation where perceptual conditions are difficult, *S* may receive meaningful cues from the stimuli and still make wrong verbal responses. The cues have the effect of narrowing the possible responses to a few, or of establishing for *S* a class or group of stimuli of which he is certain that the stimulus just presented was, or was not, a member. For a good illustration of how this happens in a perceptual situation somewhat different from ours see Craik and Vernon (1). There remains the question: What are some of the factors that influence the *S*'s verbal response in a guessing task such as this?

The work of a number of *Es* makes it clear that there are many determinants of the response in a psychophysical judgment. The sensory threshold is probably the most important factor, but there are others which have pronounced effects. Preston, for example, showed that *Ss* tend not to repeat the judgment just previously made (6). In

addition he (see also Schafer [7]) reports pronounced contrast effects in successive judgments. The probability is better than .5 that a near-threshold stimulus will be reported as not sensed if it follows a stimulus which was clearly above threshold. Subjective preferences for numbers and sequences of numbers have been found by Yule (9) and Chapanis.<sup>3</sup> And so on.

In this experiment there are several important factors that seem to have partially determined the subject's verbal response. These are:

1. *Word legibility.* This point is so obvious as scarcely to require comment. Some words are more legible than others under any given set of exposure conditions. Note, however, that this factor can operate in a negative, as well as a positive way. When a stimulus has been seen indistinctly, the *S* may tend to avoid using the names of words which are highly legible.

2. *Letter legibility.* Even more important for this experiment is the factor of letter legibility. Some letters are more legible than others under any given set of exposure conditions. (See, for example, Tinker [8].) In this study introspective evidence strongly supports the importance of this factor, as do the data on word-sequence preferences. If only the letter J was seen clearly—as sometimes happened during a trial series of exposures for the junior author—the *S* first guessed either SIJUD or CEFIJ, and, if that response was wrong, followed it with the other alternative. Sometimes it was possible to see rather distinctly that the second letter was an A—thus reducing the range of possible choices to only two.

3. *Word preferences.* Over and above the factors of word or letter legibility, there is some evidence that, in the absence of other cues, *Ss* consist-

ently preferred to use some words as responses and rather consistently avoided others.

4. *Word-sequence preferences.* Not only are certain words preferred over others, but there is good evidence that *Ss* tend to use words in certain sequences. We have shown previously that many of these sequences appear to depend not upon subjective preferences, but upon elements common to the component words. Whether all such sequential preferences are the result of common recognizable elements cannot be determined from our data. Many similar elements are evident in pairs such as SIJUD and FEXAD, CEFIJ and SIJUD, LATUK and TAROP, and so on, but with our data we can only point to these sequence preferences without explaining them. We may recall, however, that even nonsense syllables have some associative value, and that in his investigation of the Zenith radio experiments in telepathy Goodfellow (2) found very striking preferences for certain sequences of meaningful, but unrelated, words.

Now for the question: Can factors like these account for the effects found in the Lazarus-McCleary experiment? We think so. In the Lazarus-McCleary data (see Fig. 1) mean GSR's are given for four classes of wrong responses. The largest average GSR occurred after shock stimuli which the *Ss* identified as other shock stimuli, i.e., the *Ss* had placed the shock stimuli in the correct class. The smallest average GSR occurred following nonshock stimuli which the *Ss* identified as other nonshock stimuli. Intermediate and nearly equal mean GSR's were found after shock stimuli which the *S* called nonshock words, and vice versa. In this connection, note too that the average response effect is almost as large as the average "subception" effect. This means that the size of the GSR is largely, but

<sup>3</sup> Chapanis, A. Unpublished data.

not entirely, dependent on whether *S* thought (as evidenced by his first verbal response) that the stimulus was a shock word, or nonshock word.

Now let us make some hypotheses about what the *Ss* might have been doing in the Lazarus-McCleary experiment. Suppose, for example, that we had conditioned SIJUD and CEFIJ to shock and had used FEXAD, LATUK, and TAROP as nonshock syllables. Suppose further that a word is presented and that the *S* manages only to recognize an A somewhere in the word. We would expect his verbal response to be either LATUK, TAROP, or FEXAD, but in any case we would expect a small GSR, because none of the plausible responses is a shock word. Suppose, on the other hand, that *S* recognized, or thought he recognized, only an IJ somewhere in the word. His verbal response will be either SIJUD or CEFIJ, but *right or wrong*, we would expect a large GSR because the only plausible responses are shock words.<sup>4</sup>

Finally, suppose that SIJUD is presented and *S* recognizes only the final D. The plausible responses are FEXAD, which is wrong, and a nonshock word, or SIJUD, which is correct and a shock word. In this instance the first verbal response is misleading because the *S* may be perfectly aware that the stimulus may have been either a shock or nonshock word. Thus, we would expect the GSR to be moderately large because the plausible responses include both a shock and nonshock word and, so far as *S* is concerned, it could be one or the other. If *S* gives the wrong verbal response, his GSR is entered into the WN category for shock stimuli in Fig. 1. Note, however, that exactly the same kind of reasoning applies to the

GSR's in the WS category for nonshock stimuli in Fig. 1. Suppose that FEXAD, a nonshock syllable, is presented and *S* recognizes only the final D. A GSR of intermediate size would again be expected since the plausible responses still include a shock and nonshock word. If the *S* guesses wrong, his GSR is entered into the WS category for nonshock stimuli. Such cues, it seems to us, could account entirely for the relative heights of the four striped bars in Fig. 1 and, since the solid bars are averages of the striped ones, for the subception effect as well.

Lazarus and McCleary point out, and this is also evident from Fig. 1, that when the *Ss* were wrong, they showed no tendency to categorize correctly. That is, when a shock syllable was presented and *S* guessed wrong, he was not more likely to guess some other shock word. Similarly, when a nonshock word was presented and *S* guessed wrong, he was not more likely to guess some other nonshock word. Indeed, there is some evidence that the reverse occurred. This suggests that the *Ss* were categorizing on some basis other than the previous shock, or nonshock, association of the word. Thus, this observation lends further support to our interpretation that *Ss* were categorizing on the basis of common recognizable elements among the words.

In this connection, it is important to point out that the Lazarus-McCleary syllables appear to have more common elements than ours. They used YILIM, ZIFIL, JIVID, JEJIC, GA-HIW, GEXAX, YUVUF, ZEWUH, VAVUK, and VECYD. Note that three words have the letter I, an easily recognized letter, occupying the second and fourth positions. Two other syllables have an I in the fourth place. Two words each start with Y, J, G, Z, and V. Three words have a U in the fourth position, and so on. The pres-

<sup>4</sup> For evidence that conditioned responses can be evoked by elements of complex stimuli, see Pavlov (4, p. 142 f.), for example.

ence of such a great number of common elements among their syllables makes our explanation of the data more likely. We strongly suspect that cues such as these carry the information which enables us to "subceive."

#### SUMMARY

Ten Ss were shown nonsense syllables under conditions such that they could recognize half or less of the syllables correctly on the first trial. For only those stimuli which subjects failed to identify on the first trial, the mean number of additional guesses necessary to name the stimulus correctly was compared with the mean of a distribution of random guesses following stimuli which were not available to the Ss as responses.

1. Significantly fewer additional guesses were needed to identify incorrectly perceived stimuli than to make the correct response in a series of random guesses. Tests confirm the randomness of the distribution of guesses for stimuli about which the Ss were uninformed.

2. Subjects were able to guess from 15 to 52 per cent of the stimuli correctly on the first trial. The superiority of guessing incorrectly perceived stimuli over random guessing seems to hold for the entire range.

3. Subjects exhibited preferences for certain words as first responses. They were not consistent among themselves as to which words were preferred.

4. Certain sequences of words seemed to occur more often than others. Some of these sequences are composed of words which have similar elements. Recognition of these elements may be the basis for such information as is conveyed, and may provide the explanation for the results of recent experiments on "subception."

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## A MORE RIGOROUS THEORETICAL LANGUAGE

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The purpose of the present paper is to discuss the relevance of an explicit metalanguage in contemporary psychological theorizing. We shall attempt to show the importance of specifying the rules concerning meaning and denotation of terms used in theory construction, and to set forth rules for the admission of constructs as adequate for theory construction.

To illustrate some of the criticisms which we shall make, we have chosen examples of unfortunate usage in modern psychological theorizing. We do not, by this device, wish to call attention to specific theories as inadequate. Rather, we feel that these unfortunate usages are typical of much of psychological theorizing.

### PART I

1. *Theory as a form of language.* Every theory is a system of language, and as such, shares certain characteristics of all languages. The first distinction we make is between *language* and *metalanguage*.

When we use the word *language*, we refer to a system of habits or activities of human beings which serves the purpose of communication and of co-ordination of activities between the members of a group. By the word *metalanguage* we refer to a system of habits or activities which serves the purpose of discussing the use of the

language. In other words, a metalanguage is a language which has as its subject matter a language proper. Thus every language has two parts: (a) the metalanguage, which contains rules for the use of the language proper; and (b) the language proper, which is used for the purpose of communication. The metalanguage in turn is usually divided into three parts: syntax, semantics, and pragmatics (4).

The syntax of a metalanguage contains (a) an enumeration and listing of the signs of the theory; (b) rules which determine when an expression is significant, and when a significant expression is a part of the theory; (c) rules which determine when a given significant statement is a deduction from other statements of the theory. All of these tasks which belong to syntax may be accomplished without reference to the meanings of the signs and expressions of the theory (13).

The semantics of the metalanguage is concerned with problems of denotation, truth and falsehood, and meanings of the signs of the theory. The rules of the semantics determine what is denoted by the terms of a theory, when a statement in a theory is true or when it is false (4).

The pragmatics of a metatheory is concerned with the way in which the terms of the theory are conventionally used. The rules of pragmatics contain statements which describe the uses of the various terms in the language (4, 13).

Examples of syntax, semantics, and pragmatics taken from the English language are: (a) Syntax, "Every complete sentence contains a subject and a

<sup>1</sup> The authors wish to express their gratitude to their friends and professors who so kindly criticized the first draft of this paper. We especially owe thanks to Professors Henry S. Leonard and Lewis Zerby of the department of philosophy, and to Professors M. Ray Denny and Donald Johnson of the department of psychology.

verb," "An interrogative sentence is ended with a question mark"; (b) Semantics, "The word 'mother' is properly used when a female person stands in 'gave-birth-to' relation to another person," "The word 'trial' denotes a sequence of events in which some person is tested to determine innocence or guilt of some crime"; (c) Pragmatics, "The word 'ocean' is used to refer to a large body of salt water which covers a great part of the earth," "The word 'sea' is used to refer to a large body of salt water smaller than an ocean and usually surrounded by land," "The word 'lake' is used to refer to a body of fresh water usually surrounded by land."

The notion of a metalanguage has its analogy in psychological theory in the "point of view," or the "school of thought," or the "frame of reference" of the theorist as he approaches a particular subject-matter area. These terms designate particular approaches to the subject matter of psychology, e.g., Behaviorism, Purposive Behaviorism, Dynamic Psychology, etc. The different approaches as they indicate a particular semantics and a particular pragmatics represent different informal metalanguages, which serve to control the use of their distinctive terminology.

It is important to note that while all languages contain syntactic, semantic, and pragmatic rules, for any given language these rules may not be explicit. If these rules are not made explicit, it is possible for the theorist to construct statements which are not only ambiguous but are meaningless for the particular theory under consideration.

2. *Conversational language as opposed to theoretical language.* By the term *conversational language*, we shall understand any one of the vulgar languages, e.g., English, French, German, etc.

By the term *theoretical language*, we

shall understand a particular language designed specifically for the purpose of making unequivocal assertions about a given subject matter. In this distinction we follow Carnap (4).

The distinctive differences between conversational and theoretical languages, as we would like to use the terms, arise from the specificity of the metalanguage associated with a theoretical language, as compared with the relative lack of specificity of the metalanguage associated with a conversational language.

Contemporary psychology in using a conversational language with its implicit and ill-defined metalanguage creates for itself many purely linguistic problems. First, in the area of semantics and pragmatics we find ambiguity of meaning associated with particular symbols. Ambiguity arises in a conversational language because of the multitude of contextually determined meanings a symbol may have.

It is easy to slip, unnoticed, from one meaning of a word to another—to use two different expressions with the same symbol in the same discourse. As an example of the use of one symbol with more than one meaning, consider the following passages from Mowrer and Lamoreaux (11). "But fear will continue to be present between trials; and when the rat tries likewise to deal with this fear by leaping, nothing happens—the fear is not reduced" (11, p. 198). And again, "Since it is the situation as a whole which has become the conditioned stimulus for the fear reaction, any response which will remove the rat from the situation will be powerfully reinforced by fear reduction. In this way the leaping can be differentially strengthened and made to rise rapidly in the rat's hierarchy of responses to the acquired drive of fear" (11, p. 197). In another place, "the intermediate conditioned response-drive of fear."

The word "fear" is used in the above quotations with the following different meanings: (a) as a stimulus; (b) as something with which the rat attempts to deal, i.e., something experienced by the rat; (c) as a stimulus-produced drive; (d) as a response-produced drive; (e) as an emotional response; (f) as an acquirable drive. Furthermore, it is often difficult to determine from the context of usage just which one of the many meanings is meant in any given instance.

It would seem that if we are to have some degree of clarity in a theoretical language, it would be necessary to make explicit the relationship between a symbol and its meaning.

First we would like to distinguish between the terms *symbol* and *expression* by adopting a convention proposed by Lewis (8, pp. 73-74):

... Linguistic signs are verbal symbols. A *verbal symbol* is a recognizable pattern of marks or of sounds used for purposes of expression and communication. . . . Two marks, or two sounds, having the same recognizable pattern, are two *instances* of the same symbol, not two different symbols. . . . A *linguistic expression* is formed or determined by the association of a symbol with a fixed meaning. . . . If in two cases, the symbol is the same but the meanings are different, then there are two expressions; not one. Also, if in two cases the meaning is the same but the symbols are different, then there are two expressions; not one. But if in two cases . . . the symbol is the same and the meaning is the same, then there are two *instances* of the expression, but only one expression.

To be consistent with our adopted usage of the relation of a symbol and its meaning we would consider that the above uses of "fear" would represent six different expressions. The writers quoted distinguished two, " $S_f$ " and " $R_f$ ," namely (a) and (e) above, respectively; and used the word "fear"

indiscriminately with the other four. As these examples indicate, it is imperative that any theoretical language system should be so constructed that the meanings which are carried by any symbol should be unmistakable. The above examples also testify to the inadequacy of the conversational language as a vehicle for scientific theory. This inadequacy of the conversational language as a medium for psychological theory lies in the fact that the rules of the metalanguage are not specified with sufficient rigor.

Secondly, a syntactical problem concerning conversational language—the fact that the conversational language does not lend itself to deductive procedures—results in the theorist's attempting to make deductions on the basis of the *meanings of expressions* contained in the statements of the theory, instead of on the basis of the *form of the statements* contained in the theory. For a discussion of these problems see Woodger (13). Owing to the fact that the structure of the conversational language precludes the possibility of using valid deductive procedures, the theorist attempts to base deductions on meanings which may be inferred from the context and which seem to be the same. It is this similarity of meaning which leads to the illusion that such and such statements are valid consequences of such and such other statements. It is indeed interesting that few of our present-day theorists publish their deductive arguments in any rigorous symbolic form.

## PART II

1. *Introduction.* Psychological theorists have tended to make distinctions between different types of constructs which might be used in developing a theoretical language (8, 9). The following four notions have been used by psychological theorists as a basis for

differentiating between different kinds of constructs: operational definition, reality status, surplus meaning, and specificity of formulation.

We intend to analyze each of these notions separately and to show that they cannot be used as a basis for distinguishing between different kinds of constructs. Finally it will be our task in this section to formulate a set of rules which will determine the characteristics of constructs as special types of symbols that are employed in a theoretical language.

*2. Operationism and meaning.* We should like to discuss briefly two familiar notions concerning the definition of constructs. These notions are the meaning and the significance of a construct. Any construct, if it is to be of use in an empirical theory, must have meaning and must be significant to the science. To have meaning it must be operationally defined; to be significant—that is to possess explanatory and predictive value—it must be explicitly related to other constructs in the theory (1). These are to be considered the minimal criteria for the admission of constructs.

The meaning of a construct is the meaning given by the definiens of its definition and nothing more (3; 8, pp. 134-135). The use of a description of operations and their effects as the meaning of constructs serves at least three purposes. First, the meaning is specified in terms that have unambiguous denotations. Secondly, the construct is exposed to the possibility of experimental manipulation with the result that the theory thus specifies what operations must be used and what effects must be observed before the theorist may claim that the experimental results are consequences of the operation of the constructs in the situation. Finally, the definition presents adequate criteria upon which to assert or deny

the presence of the construct for explanatory purposes.

These three characteristics of the operational definition would seem to be absolutely essential for the admission of any construct into an empirical theory. If the construct is to be useful in a predictive sense, then it must bear explicit relationships to other constructs in the theory, since the prediction and explanation of phenomena is the proper task of theory. Thus, demonstration of the construct by achieving certain results with a given set of operations does not alone constitute adequate grounds for the admission of a construct into an empirical theory. The specification of the operations and acceptable results merely define the construct in question.

The present-day theoretician—insofar as Boring speaks for him—is apparently not interested in the notions of operationism and formalization. Boring says,

The reduction of concepts to their operations turned out to be dull business. No one wants to trouble with it when there is no special need. The reduction takes thought and study, and they take time, and there may be little or no gain. A still more rigorous language is furnished by symbolic logic, but no one wants to reduce James's *Principles* to a set of postulates and conclusions after the manner of Hull in his most exact moments. The operational technique seems to have become something to use when the user thinks he can get somewhere with it (2, p. 658).

We would differ with the position expressed in the above quotation in some particulars. First, the reduction of concepts may be dull business, but this is equivalent to saying that the giving of exact empirical meaning to one's concepts is dull business. Whether it is or is not a dull business, it is important that empirical concepts should have exact empirical meaning. This is achieved

by the use of the operational definition. The same remarks apply with respect to the second and third lines of the quotation. There may be no special need to give our empirical constructs meaning, and it is true that little may be gained by so doing—but the little that will be gained is the possibility of eventually constructing theories which will be testable. Second, the fact that no one wants to reduce James's *Principles* to statements in symbolic logic is not an argument against the use of symbolic logic in scientific psychology. It is an extremely interesting paradox that many psychologists look forward to the day when psychological theory will be couched in mathematical terms, yet symbolic logic, a more versatile language and one which makes few assumptions about the data, has received little attention. If we are going to express the relations which must obtain between constructs in a form which will allow a rigorous deductive methodology, then we shall be forced to the use of symbolic logic in one form or another.

3. *Reality status.* There has been some tendency in the past few years to advocate an appeal to the reality of constructs, as opposed to operational definition, as a basis for their admission in theory construction. The procedure is to assume that the constructs are ". . . actually existing structures which might eventually be described by direct experimentation. . . . Because it is assumed that these hypothetical constructs exist, and because of the intrinsic properties that they are assumed to have, the correlations between experimental conditions and results are . . . seen as necessary correlations, as inevitable consequences of the functioning of these hypothetical constructs" (7, p. 283). Or again, "genuine hypothetical constructs cannot be isomorphically related to a system of neural events,

but must be a system of neural events . . ." (7, p. 284).

It is seen that the emphasis is on the realness, the existence, or the genuineness of the construct. For example, the realness of *Dynamic Systems* (7) is derivative from the assumption of the realness of neural events. However, the meaning of the theory of neural events is given, by the physiologist, in terms of operations, or by appeals to chemistry and physics. What is meaningful to the physicist is again dependent upon operations, but what is real to him depends on his particular metaphysics. For example, Newton began his *Principia* with *Space, Time, Matter*. These were the real things of which the world was composed. Other entities were known only in terms of these three. Modern quantum physics is based on two assumptions (as per Einstein); these are space-time (field) and matter (5). Still other theoretical physicists have constructed other metaphysical systems which do not mention the word "matter," but which are satisfactory for the development of the concepts of the science. See especially Whitehead (12).

It seems that what is considered real in one science is given meaning in a pre-supposed science in terms of operations, and presuppositions about the subject matters of still other sciences. In the final analysis, meaning is given in any science in terms of operational definition—only after the theorist has arbitrarily decided what are the real things to which he wishes to reduce his concepts. We would conclude that Hull's  $gE_R$  (6, p. 242) is just as real as Krech's *Dynamic Systems*. Indeed, more real—Hull's  $gE_R$  has been, at least in part, operationally defined and hence has empirical meaning.

Individuals who distinguish types of constructs on the basis of reality status seem to forget that theories are sets of

communicable signs. The operational definition is simply a device to transfer sense meanings from one group of symbols to another. The meaning which is conveyed by the *definiens*—the description of events in the real world—is transferred to the *definiendum*, the symbol which denotes the construct in question.

We feel that each theorist has a right to make any assumptions whatsoever about what is real and existent in the world. These metaphysical assumptions are prior to the particular empirical system which is to be constructed. For the empirical scientist, since the constructs are operationally defined in terms of "reality," the only criterion of an adequate metaphysics is the predictive efficiency and the explanatory power of the resulting empirical theory.

By *predictive efficiency and explanatory power* we mean one and the same thing—the ability to deduce statements descriptive of phenomena as valid consequences of statements in the theory. The distinction between prediction and explanation lies not in things that the theorist does, but rather in the intent of the theorist. In both cases the theorist does the same thing—he attempts to deduce statements which describe empirical phenomena as valid consequences of a theory. In the case of *prediction* the intent of the theorist is to *test* the theory. In the case of *explanation*, the intent of the theorist is to *understand* the phenomenon. Therefore to assert that a theory is to aid in prediction and explanation is to say only one thing, not two. Furthermore, by the phrase "deduce as valid consequences" we mean deduction which turns on the form of the statements, and not deduction which turns on the fact that two sets of statements contain words (expressions) which may be construed in the same way.

#### 4. *Surplus meaning and specificity of*

*formulation.* By surplus meaning we refer to the notion that a construct may have meaning beyond that stated in its definition or in the laws which relate it to other constructs in a theory (9). The notion of the surplus meaning of constructs arises from at least three sources: first, the assumption of the reality status of constructs; second, the use of discussion instead of operational definition as a method of assigning meaning to constructs; third, the use of conversational language.

The assumption of reality status allows one to postulate an unknown set (yet-to-be-discovered set) of properties and relations as possibly influencing any given set of obtained data. Further, there is no way to determine which constructs may be legitimately said to be operating, hence there is no way to make proper deduction of expected experimental results. In fact, it makes the prediction of phenomena on the basis of prior deduction from independent fact impossible. This, of course, leads to *ad hoc* explanation—which must, by the nature of the assumption of surplus meaning, be the only form of theorizing possible. The outcome of such "ad hoc" ism is apparent when one views the stagnation of psychoanalytic theory from the point of view of scientific endeavor. A theory cannot proceed from undefined gross analysis to a precise quantified science. For an opposing point of view see Marx (10). The nature of the formulation of such theory prevents development. It precludes the possibility that the theory will be able to predict erroneously—thus the theory is always supported, regardless of experimental outcome.

The use of discussion instead of the operational definition to get across what is meant by a given construct places undue emphasis on rational or intuitive interpretation of experimental results, and leaves any operational defini-

tion of the construct up to the experimentalist who wishes to work with it. Whether the construct is thus correctly formulated and interpreted in a given experimental situation is not determined by the theory of which the construct is a part. As a consequence, claims of misrepresentation, distortion of meaning, and destruction of purpose, are occasionally leveled at the experimentalist. This amounts to a state of authoritarianism in the less rigorously formulated areas of psychology. If meanings are assigned by operational definition, the theorist leaves only the task of validation to the experimentalist. The accuracy and success of a theoretical formulation are open to the public—indeed, even to the neophyte in psychology!—for confirmation or denial.

The third source of surplus meaning lies in the use of conversational language as a method of conveying meaning. The surplus meaning is admitted through the fact that the words (expressions) of a conversational language have meaning in context, meaning which varies as the words are used in different sentences. The use of constructs having operational meaning is simply a device for achieving fixed and precise meaning in a communicable and deductively manipulable form, and eliminating surplus meanings and their consequences.

In view of the above discussion we state the following rule of procedure: *To be testable a theory must be formulated as complete at the time of the test.*

5. *Rules for the admission of a construct.* Keeping the above discussion in mind, we should like to list what seem to us to be necessary and sufficient conditions for the admissibility of constructs in theory construction. We do not wish to imply that any construct will necessarily be successful if it meets the criteria we shall set down. Success

or lack of success is to be determined by empirical test of the content of the theory in question. The constructs in a theory must meet the following criteria if the theory is to be testable.

First, a construct which is adequate for use in empirical theory construction is operationally defined. The operational definition contains *a description of manipulations which are performed in the laboratory, and a description of the effects of these manipulations upon the behavior of the experimental subjects.*

Second, the concept under consideration must bear *explicitly stated relationships to other constructs in the theory of which it is a part.*

Third, a construct must *vary unidimensionally and continuously, and must affect at least one abstracted aspect of behavior, such that the behavior will vary unidimensionally and continuously.*<sup>2</sup>

<sup>2</sup> We define measurement as the assignment of numbers as names of members of a range of attributes which objects possess, such that the magnitude of the number reflects the magnitude of the attribute possessed by the object. It will become obvious that with any given measurement procedure we can work with one and only one range of attributes at a time. This is to say that one measurement procedure will define different numbers in one and only one class of qualities. Measurement consists in discovering which member of a class of qualities is possessed by a given object of measurement. Thus if the construct in question is to be testable, it must conform to the assumptions of the real number system. This is the reason we assert that a construct must vary unidimensionally. The construct will take different values in different situations, but all values will represent (name) qualities which are members of only one class of qualities.

In addition to the above there is the further consideration that where behavior is complex and discontinuous, there the theory will also be complex. If we observe a discontinuity in behavior in a given situation, it is reasonable to assume that the behavior is not a manifestation of a single construct.

As an elaboration of certain implications of the first and second criteria above, we list the following two statements: (a) To avoid circularity, a construct must be used only to predict behavior which is independent of its definition. (b) The construct is logically prior to the behavior which it predicts.

The notion of logical priority makes no assumption about temporal priority, i.e., antecedent events. The notion of temporal priority is not to be taken as a restriction upon empirical constructs. Its use as a restriction upon empirical constructs arises from thinking about theory from a particular point of view; namely, the metaphysical assumption of a causal relation between antecedent events and consequent events, and from the pragmatic utility of predicting future events. The only restrictions we wish to make are that predictions about behavior shall be deducible from the laws from the theory, and that the particular constructs which mediate the deduction be defined independently of the behavior which they predict.

These conditions for acceptable constructs are not new with the present authors. They are commonly found scattered throughout the philosophy of science, and in discussions of procedure in psychology.

In closing, we should like to remark that empirical science, indeed, science in the inclusive sense, is an activity of human beings. The constructs with which the empirical scientist works are the products of his own imagination, imagination which is guided by knowledge of empirical events. The criterion of a successful theory is its adequacy as an instrument for prediction and explanation. Nothing is inconceivable, if it allows us to predict successfully and accurately. The constructs with which the empirical scientist works have those

properties, and only those properties, which the scientist gives them in any particular formulation of a theory. Failure to predict successfully is an indication that the theory is either inadequately formulated, or its empirical content needs to be changed in some particulars. But, to assume that constructs have surplus properties which are not accounted for is another way of saying, "Psychology is an art, not a science."

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## THE BRAIN ANALOGY: ASSOCIATION TRACTS<sup>1</sup>

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There are four phenomena that indicate a need for a more complex cortical structure than that shown in the original Brain Analogy paper (2, p. 156). These phenomena are secondary conditioning (4, p. 33), conditioned inhibition (2, p. 175), sensory preconditioning (1), and transfer of differentiation (4, p. 228). The present paper uses the first three phenomena as examples. Conditioned inhibition can be explained by the original structure, but since secondary conditioning cannot, and the two phenomena have almost identical experimental programs, both are described in terms of the new assumptions.

In addition to the above there are several other facts which indicate that the original Brain Analogy (BA) hypothesis is not without its difficulties. Pavlov has noted that repeated application of tone *A*, say, does not result in differentiation unless the method of contrast is used—tone *B* without reinforcement (4, p. 117). The original hypothesis (2, p. 173) easily meets this test in the laboratory. But transfer the BA to a natural environment between experiments and differentiation takes place without the formal method of contrast because other specific neurons and the generalized neurons are actuated by many natural auditory stimuli which, of course, are not reinforced by the appropriate US. In time, even the specific conditioning to tone *A* would be lost.

An interesting fact deduced from the above situation is that monopolar neurons are relatively useless for complex

animals. Since it is a fact that laboratory conditioning remains fairly stable in magnitude when exposed between experiments to natural stimuli, most neurons are probably bipolar with one pole representing some uncontrolled factor in the laboratory. The absence of this factor between experiments prevents extinction.

Generalized neurons are inevitably involved in any problem concerning differentiation and their properties are necessarily related to the complexity of the environment. A simple mechanism suffices for a simple environment but added factors require a more complex structure for both generalized and specific neurons.

These facts indicate that the difficulties in the original hypothesis were of degree rather than principle—the machine's IQ was too low to cope with two environments. Since intelligence increases with the resolving power of the analyzers (3, p. 456), intelligence is low where monopolars predominate. However, this paper is based on a laboratory environment which adequately illustrates certain principles.

The original BA structure allowed conditioning from sensory neurons to motor neurons. The new BA structure has the additional property of permitting conditioning between sensory neurons. A sensory neuron is now defined as a unit consisting of one or more receptor elements or poles, a latency element in each pole, a combining element, a cell body, and a multitude of branches each terminated by a delta cell which connects with a motor cell body or with the cell body of another sensory cell. A generalized neuron is defined as a unit

<sup>1</sup> The author wishes to express his gratitude to Vera Jane Coburn for making the drawings and for assistance in preparing the manuscript.

consisting of a multitude of receptor elements having identical properties, including latency, a combining element, and a multitude of branches each terminated by a delta cell which connects with the cell body of a sensory neuron. The connections to sensory cell bodies are known as association tracts (AT's). Because the mechanism of generalization is restricted to the AT's, it can affect the motor cells only in an indirect manner through conditioning to sensory cells (AT conditioning) which in turn connect with the motor cells (sensory-motor, or SM, conditioning). The purpose of this provision is to permit transfer of differentiation with little or no interference by generalization.

The AT's make it possible for two signals to become associated more or less independently of the activity of the motor cells—individually of conventional reinforcement.

In the original paper conditioned inhibition was attributed to the so-called zero-ratio bipolar neuron (2, p. 175). This naïve notion, suitable for a first approximation, is quite limited in application. We now observe that since thresholds have normal distribution (2, p. 157), any bipolar neuron which has an inhibitory pole stimulated at a sub-threshold level, in effect complies with the zero-ratio postulate. But there is an additional advantage in that the available population of *nonactuated* bipolar neurons is roughly proportional to the stimulus strength at the "zero" pole. This is the mechanism which allows conditioned inhibition to function best when the conditioned inhibitor is strong (4, p. 74) because a neuron that is not actuated on a nonreinforced trial possesses stable conditioning and the strong stimulus insures a sufficient population of these. A weak stimulus causes an insufficient population of stable bipolar neurons, allowing competi-

tion by gamma phase differentiation<sup>2</sup> (2, p. 170) of monopolar neurons to dominate the response. Secondary conditioning then reveals itself through means of AT connections.

Although the new structure is more complex, the principles are familiar to readers of the first paper (2) and the burden of additional complexity is more than offset by the increased scope and power of the technique. While this paper is limited to only three examples, one can easily visualize probable applications to such theoretically important subjects as latent learning, token rewards, and others.

#### THE STRUCTURE

The analysis is somewhat simplified by the fact that the experimental program automatically collects certain groups of neurons into functional clusters and eliminates others from consideration. But this is offset by the existence of several hundred groups. Since an exhaustive analysis is possibly unnecessary at the present time, precision in proof is sacrificed for convenience in exposition.

Every problem involving two overt stimuli can be placed in one of two categories: The overt stimuli actuate certain *receptors* in common; or, they do not. If overt A and B are dissimilar enough, auditory and visual for example, to avoid actuation of any common receptors as shown in Fig. 1, the number of groups is a minimum. In this paper, generalization is assumed insufficient to permit either overt stimulus to act as a substitute for the other.

Neurons with two excitatory poles must have both actuated to get a response from the combining element, but the neurons with an inhibitory pole will respond only when the threshold of the

<sup>2</sup> Formerly called *periodic reconditioning* (2, p. 162).

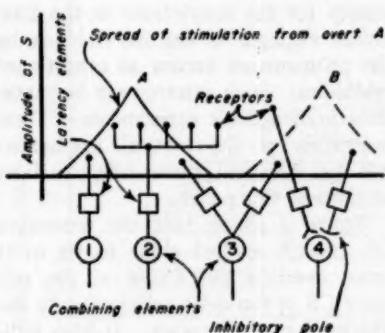


FIG. 1. The location of the receptors in the figure is significant as to threshold and position in the sensory field. The receptor of the excitatory pole of neuron 4, for example, has a relatively high threshold while the receptors of 3 have low thresholds. The excitation of any receptor is usually a function of stimulus position as well as intensity.

inhibitory pole is not exceeded. However, in every bipolar case due consideration must be given the latency elements. Simultaneous stimulation of both poles is not an adequate criterion: the effect of the stimulus must be simultaneously present at the combining element.

Each group, represented by a single neuron in the figures, consists of many individual neurons with the usual distribution of properties (2, *Postulates*) except, of course, that if a group has specified threshold limits, it represents only a particular fractional part of the area under a normal distribution curve. It is essential to understand that any alteration in an experimental program, such as a change in magnitude of *A* or *B*, reclassifies many of the groups and transfers some to other functional clusters.

Certain neurons, sometimes structurally dissimilar, are functionally identical in a particular experimental program. Neurons 1 and 2 of Fig. 1 are functionally identical, except possibly for latencies, because they are both

actuated whenever *A* is present and neither is ever actuated by any other phase of the indicated stimulus situation. In general, then, we can classify the neuron groups according to their behavior in the experimental program without concern for their structures. These functional clusters are relatively few in number.

There is, however, one outstanding peculiarity of the new mechanism: The AT conditioning has little effect on the motor response unless the program is altered.<sup>3</sup> The experimental alteration, of course, is the test trial which reveals the existence of AT conditioning. The cause of this indifference to the presence of the AT's is found in the program itself. Any AT conditioning to sensory cells which are *not* reinforced by the US is "open circuited" at the motor cell body whereas any AT conditioning to sensory cells which *are* reinforced is superfluous. But let a change be introduced into the program and the presence of AT conditioning is immediately apparent. For example, as long as AT activity is accompanied by SM activity, there is little or no experimental evidence that the CR is partly due to the former, but when the latter is absent, as on a test trial, the CR is still present and attributed by the hypothesis to AT conditioning. The interference of pulses in the sensory cell body and the reduction in the *E* function (2, p. 171) due to extra stimulation, combine to maintain an output pulse rate that is considerably less than the sum of the inputs. It is concluded that with a constant program the AT's are without significant effect on observed behavior. Figure 4 will aid in analyzing the above argument.

<sup>3</sup> The conditioning and extinction properties for AT connections are informally assumed to be the same as for SM connections. The latter are given in detail in the original BA paper (2).

It is also seen from examination of Fig. 4 that if the program is arranged so that AT conditioning occurs from each of two sensory neurons to the other, and this is possible, a regenerative or positive feedback circuit is established which might continue to operate once started. No assumptions have been made concerning latency in the AT's, but it is apparent that a finite value exists which could cause a train of pulses to appear in the circuit. However, a short conduction time causes the initial pulse to be returned to the starting neuron while it is still refractory following the generation of the pulse (2, *Postulate 10*) and immediately stops the feedback action. Since the delta cell is not reinforced, extinction ultimately takes place, breaking the feedback circuit.

#### SECONDARY CONDITIONING

Secondary conditioning has been defined by Pavlov (4, p. 33) as the CR which develops when a neutral stimulus is followed by a CS that is not reinforced on the occasional secondary-conditioning trials. After practice the neutral stimulus, alone, evokes the UR. It is an essential condition that the secondary CS be weak in fact or in effect (by delay in applying primary CS). The experimental evidence is based on dissimilar overt stimuli as in Fig. 1. Tone *A* is the primary CS while *B* (visual) is the secondary CS. The US is never paired with *B* which is followed by *A*, alone.

In the experiments reported by Pavlov the primary conditioning was established prior to any secondary-conditioning trials. Brogden's work on sensory preconditioning (1) shows that if the secondary conditioning is established first, the secondary CS does not have to be relatively weak and terminated prior to the start of the primary CS. The BA analysis reveals that the ne-

cessity for the restrictions in the Pavlovian example is the interference by the phenomenon known as conditioned inhibition. Such interference is impossible in Brogden's experiments. These properties of the cortical mechanism will be discussed more fully in later sections of this paper.

Figure 2 shows that the secondary CS, though applied alone in its overt form, overlaps the action of the primary CS at the delta cells owing to the distribution of latencies. It also indicates that the effective duration (2, p. 159 and 3, p. 457) of *A* is prolonged for the same reason. Although the latency distribution function is continuous (2, *Postulate 5*), we have simplified the analysis by assuming that latencies take a discrete set of values, each one of which includes a number of neurons. Figure 2 is based on the latency ogive (2, Fig. 6) with the ordinates divided into steps of 2 per cent each, but with a linear time scale.

Since the short-latency neurons of the secondary CS ( $B_1$  to  $B_{44}$  incl.) are never actuated simultaneously with the primary CS, they can never become conditioned and are henceforth excluded from the discussion. The long-latency secondary CS ( $B_{48}$  to  $B_\infty$  incl.) are excluded because they are actuated subsequently to those primary CS ( $A_1$  to  $A_{41}$  incl.) which are actuated early enough in the program to become conditioned to the US: conditioning may occur between long-latency secondaries and long-latency primaries ( $A_{42}$  to  $A_\infty$  incl.), but this fact is immaterial because the motor cells cannot have stable connections except with delta cells which have their activity initiated prior to motor activity (2, p. 169). Backward conditioning is excluded. Our concern, then, is only with medium-latency secondaries ( $B_{45}$  to  $B_{47}$  incl.), short-latency primaries ( $A_1$  to  $A_{41}$  incl.), and forward conditioning.

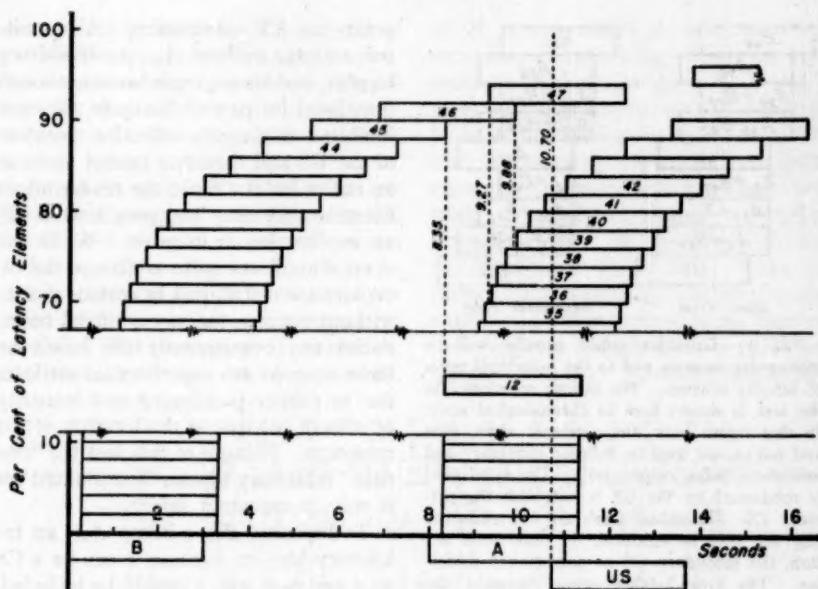


FIG. 2. Because sensory neurons have both latency elements and AT's there are many possible combinations of channels for signals in their traverse of the cortical mechanism. On the other hand, since conditioning is possible only when the appropriate contiguous elements are simultaneously in action, many other channels are closed by any particular experimental program. This figure is the Brain Analogy version of the long-established "persistent trace" concept applied to two stimuli to show how the long latency neurons of one stimulus are simultaneously in action with short latency neurons of another stimulus.

The latencies shown in Fig. 2 apply to the poles of the four types of neurons considered in this paper: Monopolar, generalized, excitatory bipolar, and inhibitory bipolar.

Because the latencies overlap in various degrees, certain groupings are possible on a basis of latency. Aggregate  $B_{45}$  can be a CS for  $A_1$  to  $A_{11}$  inclusive, and to  $B_{46}$ , while  $B_{46}$  can be a CS for  $A_1$  to  $A_{38}$  inclusive, and to  $B_{47}$ , which in turn is a CS for  $A_{38}$  to  $A_{41}$  inclusive. On the other hand,  $A_1$  to  $A_{35}$  inclusive can also be CS for  $B_{47}$ . These relationships assume monopolar neurons. Bipolars with two excitatory poles will be actuated for no longer than the duration of the shorter stimulus and usually for less time because of failure of the timing to be optimum.

Bipolars with one inhibitory pole will usually be excited part of the time unless the stimulus for the inhibitory pole is of greater duration or precisely timed. Of course if the inhibitory stimulus has less duration than the positive stimulus, no inhibitory bipolar can be suppressed for the entire cycle.

The analysis now proceeds on the basis of a restricted sample of the latencies shown in Fig. 2. Suppose we consider only  $B_{47}$ ,  $A_{38}$ , and  $A_{43}$ . The temporal relationship of monopolars, bipolars, and reinforcement is shown in Fig. 3. The cross-hatched areas are of particular significance: these are the inhibitory bipolars that are not actuated on nonreinforced trials and hence possess stability. It will be observed, however, that the inhibitor,  $B_{47}$ , does not

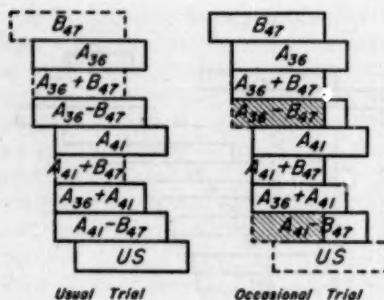


FIG. 3. Latencies apply equally well to monopolar neurons and to the individual poles of bipolar neurons. The sample mentioned in the text is shown here to chronological scale. In this figure only, the algebraic signs, plus and minus, are used to indicate excitatory and inhibitory poles, respectively. The usual trial is reinforced by the US to establish the primary CS. Occasional trials are nonreinforced (by the US) to establish, by differential action, the secondary CS or conditioned inhibitor. The cross-hatched areas represent the nonactuated portion of a trial where failure of reinforcement is without effect; these are relatively stable. But for successful conditioned inhibition the duration of *B* must exceed *A* to prevent "unmasking" of the inhibitory bipolar and resultant secondary CR.

suppress all of the action of  $A_{36}$  and  $A_{41}$  because the former lacks effective timing or sufficient excess duration to compensate. The equal duration of overt *A* and *B* was intentionally chosen to illustrate this effect. The inhibitory bipolars, then, are only relatively stable as a group; but the possession of a sufficiently long gamma phase renders them unconditionally stable in an appropriate program and the presence of inhibition for part of the cycle makes the gamma-phase requirement easy to meet.

Omitted from Fig. 3 are three generalized neurons, one of each latency, which receive stimulation identical to that given the corresponding monopolars—physical separation of *A* and *B* (see Fig. 1) prevents differential competition between generalized neurons and mono-

polars for AT conditioning. Also omitted are  $A_{36}$  without  $A_{41}$ , an inhibitory bipolar, and its converse because though functional for part of the cycle, they are inhibited during the effective duration of the US and therefore cannot serve as an outlet for *B*. Since the reader might question why they are postulated at all, an explanation is in order. While the overt stimuli are quite arbitrary, the receptors are distributed in various tissues without concern for any artificial boundaries and consequently the labels on these neurons are experimental artifacts due to chance positioning and intensity of stimuli relative to the location of the receptors. Because of this fact the "one field" inhibitory bipolar is accounted for if only to exclude it safely.

Technically,  $B_{47}$  without  $A_{41}$ , an inhibitory bipolar, because it can be a CS to 1 and 6 of Fig. 4 should be included. However, since it is never reinforced by the US and is essentially the same as  $B_{47}$ , it is omitted to simplify the figures. The case of  $B_{47}$  without  $A_{36}$  is different: it is always inhibited by the time any other neuron in the sample is excited and therefore properly excluded.

And, finally, we have the *A* or *B* monopolar category. In this program the two cases are excluded because they represent an impossible condition with fields of stimulation that do not overlap (Fig. 1).

Starting with 17 cases, we have eliminated 5 for necessity and 4 for convenience. The remaining 8 with the US are shown in Fig. 3. However, in Fig. 4 we introduce one of the generalized neurons to facilitate understanding. The conditioning symbols are the same as used previously (2, p. 174) and are not repeated here since the present paper is unintelligible without the first. It is important to understand that the individual neurons in Fig. 4 represent functional clusters rather than structural counterparts. An inhibitory bipolar, for

example, is functionally a monopolar if the inhibitory threshold is not exceeded. An experimental program may reduce a complex neuron to one of simpler function but, of course, the reverse is never true.

It will be seen in Fig. 4 that the experimental program develops many AT conditioned connections as well as the SM connections. As noted earlier, the AT conditioning has little effect on the motor response unless the program is altered. The experimental alteration, of course, is the test trial—application

of *B*, alone—which is used to reveal secondary conditioning. Since the connections from 4 through 1 to 2 and 5 must be both functional and actuated to have any effect on the motor cell, their existence is hidden by the presence of *A* on every trial except the test trial. In addition, neuron 4 is never actuated, except for the test trial, under conditions which cause it to influence, independently, conditioning between 1 and the motor neuron because the program forbids the required sequence of events. Another point of sig-

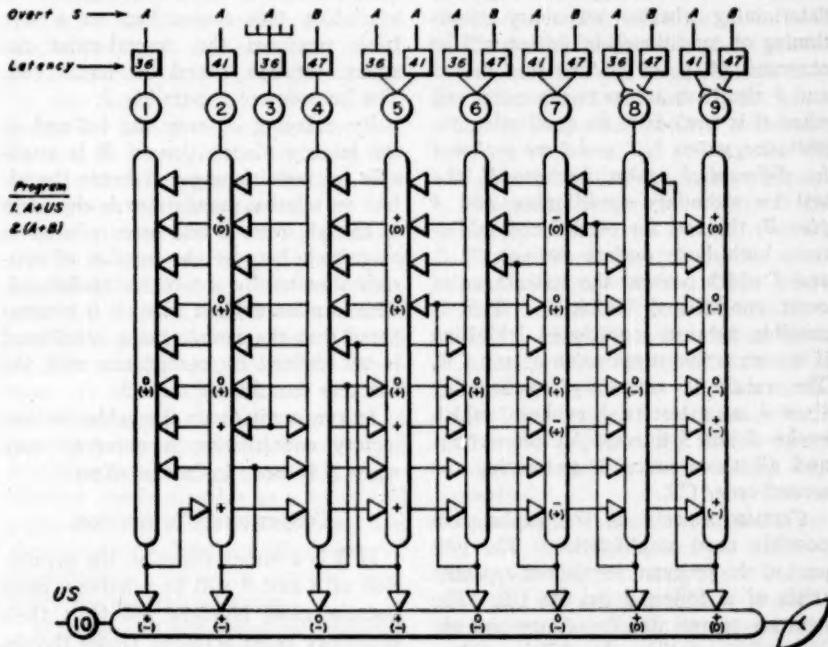


FIG. 4. The individual neurons in this figure represent functional clusters with the usual threshold variations. As *B* is varied in strength there is a shift in relative populations. This variance, in conjunction with the existence of the AT's and other factors, is the responsible agent in determining whether secondary conditioning or conditioned inhibition will be observed. It should be noted that the AT delta cells transmitting the output of any sensory cell are actually in parallel, not apparently in series as shown. The conditioning symbols used here really apply to a case where the duration of *B* considerably exceeds *A*. The significant point is that no matter what the temporal relationship of overt *A* and *B*, provided *B* starts first and has sufficient duration, there are always combinations of latencies for inhibitory bipolars which keep some of them totally inactive whenever the conditioned inhibitor is presented as part of the program.

nificance is the fact that no conditioning occurs from 4 to 8 or 9.

The circumstances of the program are such that neurons 1, 2, and 5 are in competition with 8 and 9 for control of the motor neuron. If *B* is weak, the population of 8 and 9 will be small—not structurally, but functionally. The inhibitory bipolars that have inhibitory thresholds in excess of the overt *B* are functionally identical with monopolars excited by *A*, alone. It is seen then that the populations of 1, 2, 5, 8, and 9 vary relatively with stimulus intensity; this variance is the responsible agent in determining whether secondary conditioning or conditioned inhibition will be observed (4, p. 69). The fact that 8 and 9 also have access to the motor cell when *B* is weak is of no qualitative importance, unless 1, 2, and 5 are excluded by differential action, because *B*, the test for secondary conditioning, and *A plus B*, the test for conditioned inhibition, both have outlets through 1, 2, and 5 which prevent the appearance of overt conditioned inhibition. This is possible because conditioned inhibition is not an active suppression of any CR. The result is secondary conditioning since 4, on a test trial, evokes 1 which evokes 2 and 5 through AT connections and all three unite in generating the second-order CR.

Certain aspects of the explanation probably need amplification. The first part of the program consists of repeated trials of *A* followed by the US. The neurons represented by 2 are the objects of competition by AT conditioning from 1, 3, and 8, which in effect, are components of a compound stimulus as viewed by 2. We have no data upon which to assign relative strengths to the components except to observe that since overt *B* has been assumed to be weak, 8 must be a very small component because 8 represents only a small fraction of the sum of the populations of 8 and

9: the sample fails to indicate that both 1 and 8 represent a few of the shorter latencies of their respective categories. Most of 2, therefore, are appropriated by 3 and actuation of the latter reveals generalization. The connection from 8 is insignificant and the tract from 1 is of intermediate importance. The last part of the program consists of the aforementioned trials interspersed with occasional trials of overt *B* followed by *A*. Because nearly all of the 2 group have already been appropriated, AT connections from 4 are disabled and denied access to 2. But access to 1 is available; this connection, on a test trial, produces the second-order response through 2 and the motor cell. The behavior of 5 parallels 2.

By delaying *A*, only the tail-end of the latency distribution of *B* is available for conditioning and hence the effect on relative populations is the same as though overt *B* had been reduced in magnitude because the number of neurons actuated by *A* remains unchanged. This is more evident when it is remembered that the sample being considered is one defined by coexistence with the effective duration of the US.

In conformity with the evidence, secondary conditioning is observed only when *B* is weak in fact or effect.

#### CONDITIONED INHIBITION

If *B* is a strong stimulus, the population of 8 and 9 will be relatively large because more bipolars will have their inhibitory poles actuated (more thresholds will be exceeded) and therefore a greater population will fail to respond on nonreinforced trials. These have stability. On the other hand, 1, 2, and 5, sometimes actuated without reinforcement, lack stability, and being of relatively small population cannot depend on gamma-phase differentiation which incurs an additional loss of conditioned

delta cells. They surrender control to 8 and 9.

Overt *B* now has no outlet through 1, 2, or 5 and its application, alone, excites no overt response. When *A* is applied, alone, the response is due to 8 and 9, not as originally established through 1, 2, and 5. In addition to the direct action of 8 and 9 through its own receptors, there is a possible action through AT conditioning from 1 and 3 to 9, but these unstable connections are gradually displaced by AT conditioning from 8. When *A* and *B* are actuated in conjunction, there is no overt response. This last is called *conditioned inhibition* though it has not been shown to be an active suppression of any CR. On the contrary, *the original CR is driven to extinction, rather than suppressed, by a competitor possessing stability and in a process identical with other forms of differentiation.*

It is apparent that conditioned inhibition is favored not only by a strong conditioned inhibitor, in conformity with the evidence, but also by one of considerable relative duration. The latter fact appears to have escaped attention. It is equally apparent that AT conditioning plays a significant part in the isolated phenomenon of conditioned inhibition by causing the block of inhibitory bipolars to act as a functional unit—the AT conditioning from the shorter-latency bipolars displaces AT conditioning from other neurons, especially the generalized group.

If the animal were prepared surgically or by suitable nerve blocks in such a way that influence by the musculature system (kinesthetic receptors, etc.) could not influence the experiments, it seems probable that a smooth transition would occur between conditioned inhibition and secondary conditioning as *B* was varied in separate experiments from one intensity extreme to another. We are concerned, of course, with the sali-

vary reflex. Pavlov has reported the existence of "defense" reactions at the transition intensities (4, p. 69); these, by alteration in the *Q* and *E* functions (2, *Postulates 12 and 15*), possibly reduce the amount of conditioning in both groups to an extent that makes observation difficult.

#### SENSORY PRECONDITIONING

Sensory preconditioning has been defined by the experiments of Brogden (1, p. 325) as the conditioning which forms between two stimuli actuated simultaneously without conventional reinforcement. The existence of the bond is revealed later when one of the stimuli is used as a CS with some US to develop a CR, and a test trial of the other stimulus then also shows a connection with the motor cell by exciting the UR with which it had never been associated.

Following the general scheme of the preceding sections we assume *A* is the auditory stimulus and *B* is the visual one. Each trial consists of the simultaneous actuation of *A* and *B* for three seconds. After a number of such trials *A*, alone, is followed by shock, the US, for enough trials to establish conditioned leg flexion. A test trial with *B*, alone, now produces leg flexion—a response with which *B* never had been associated.

Our sample this time consists of, say,  $A_{40}$  and  $B_1$ . The subscripts represent the 2 per cent ordinates, as before, except that the starting time of *A* and *B* is simultaneous. In the first set of trials AT conditioning forms from  $B_1$  to  $A_{40}$ . In the second set of trials SM conditioning forms from  $A_{40}$  to the US. A test of  $B_1$  then evokes  $A_{40}$  which in turn evokes the UR.

It will be noted that this hypothesis is not limited to the simultaneous action of overt *A* and *B*. They can be successive as in Fig. 2. Suppose  $B_{47}$  conditions to subsequent  $A_{40}$  in the AT's

as before. Then if overt *A* is conditioned to the US, application of overt *B* will evoke the UR through AT conditioning. But if the role of *A* and *B* is reversed, the temporal relationship becomes more critical though not qualitatively different. Again following Fig. 2 we see that a short-latency *A* will condition to a subsequent long-latency *B*. Then if overt *B* is conditioned to the US, application of overt *A* will evoke the UR through the AT's, but only if the program had been arranged so that the US followed the beginning of the long-latency *B*; otherwise, the motor cell would be appropriated by a short-latency *B* to which *A* is not a CS.

Brogden's experiments were concerned with a musculature reflex and with avoidance learning. In the early stages of such conditioning many CR's form (3, p. 458) and it is only with practice that a particular CR heads the hierarchy. When the other CR's become extinct, they "open-circuit" the corresponding AT conditioning so that the differentiation is evident from the first test trial.

It was noted in connection with secondary conditioning that while inhibitory bipolars of sufficient population convert the phenomenon into conditioned inhibition, such interference is impossible in Brogden's experiments. This is true because the inhibitory bipolars corresponding to *A* without *B* are not actuated in the first set of trials which establish AT conditioning; in the last set of trials they compete with *A*

for control of the UR but not in any differential manner that could cause one or the other to show instability. However, the fact that these inhibitory bipolars appropriate some of the motor cells is not without effect: The outlet that *B* would otherwise have through AT conditioning to *A* and US is correspondingly reduced. This agrees with Brogden's observations but, of course, we have not shown that this is the sole cause for the reduced response from *B*.

#### CONCLUSION

The AT postulates appear to offer significant advantages in correlating behavior phenomena; however, they present so many unexplored and inadequately explored facets that we can hardly expect freedom from error. But if they are fundamentally sound in principle, it seems probable that they can be improved in detail. Possessing a physical basis like the other Brain Analogy concepts, they provide all the research advantages of a *tangible* structure.

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## ONE- AND TWO-TAILED TESTS

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Hick, in "A Note on One-Tailed and Two-Tailed Tests,"<sup>1</sup> purports to vitiate the recommendations made by me in a recent paper.<sup>2</sup> I think a reply is indicated.

1. My paper was not intended to precipitate a controversy over the philosophical foundations of mathematical statistics. It *was* intended to show that, by taking advantage of available statistical techniques, the experimenter could increase the precision of his investigation by recognizing and minimizing the incidence of Type I and Type II errors. The paper was expository rather than argumentative, a restatement rather than a presentation *de novo*. All statements made therein relative to the testing of hypotheses have the acceptance of statisticians generally.

2. The experimenter's decision to adopt a particular level of confidence is not made with reference to statistical considerations. Such a decision depends entirely on the assessment of the likelihood of Type I and Type II errors and the practical importance to be attached to their occurrence. Thus, the propriety of the one- or two-tailed test depends only upon the nature of the hypothesis to be tested—not on the level of confidence adopted.

3. Although logic is timeless, the "personal equation" of the experimenter is not. The legitimate use of particular data for the test of a particular hypothesis *does* depend on why (the basis for), if not when, the hypothesis was

formulated. Hick says, ". . . it does not and cannot matter whether a theory (read hypothesis) was conceived before, during, or after the experiment; it may be suggested by the data, or it may be revealed in a dream." Now this statement is true in *all* respects if conclusions about the tenability of the hypothesis are to be restricted to the data at hand. But if these conclusions are to be extrapolated, we must strike the phrase, "it may be suggested by the data." For consider, with reference to any particular data, if the hypothesis is to proceed from the data, the best hypothesis is that the data are as they are. In such case, we might solemnly aver that they are as they are by definition, and never bother to test any hypothesis. We shall always be right, if only we select the hypothesis carefully enough—until the next time!

4. Hick challenges, at least implicitly, the Neyman and Pearson theory as it applies to critical regions. The pros and cons of such a controversy have no place in this discussion. However, it may be stated that, when the critical region selected is appropriate to the hypothesis being tested, then the level of confidence (by definition) is known exactly, and the probability that a Type II error will occur (false acceptance of the null hypothesis) is minimized.

5. Hick's remark on my treatment of the chi-square test has merit. Technically, the term "two-tailed," as applied to the chi-square distribution, refers to the actual tails of that distribution—i.e., the regions of exceptionally large and exceptionally small values of chi square. However, it is still legitimate to cut the level of confidence in half

<sup>1</sup> W. E. Hick, A note on one-tailed and two-tailed tests. *Psychol. Rev.*, 1952, 59, 316-318.

<sup>2</sup> M. R. Marks, Two kinds of experiment distinguished in terms of statistical operations. *Psychol. Rev.*, 1951, 58, 179-184.

when we predict direction of frequency discordance. This is so because when we eliminate exactly half of the possible values of chi square (that half corresponding to frequency discordances in the unwanted direction), we also eliminate half of the Type I errors.

6. The same remarks apply, with somewhat less force, to the indicated use of the  $F$  test. Bendig<sup>8</sup> has pointed out that only one tail of the  $F$  distribution is tabled—i.e., values of  $F$  equal to or greater than unity. When I discussed the increased precision which could be achieved by predicting the hierarchy of magnitude of the means,

I was referring to the fact that, in the case of simple classification analysis of variance with only two columns (when  $t^2 = F$ ), the number of Type I errors is cut in half when the negative values of  $t$  are eliminated beforehand. The number of Type I errors would be reduced still further in the case of more than two columns (variables) if the ordering of the means was predicted beforehand, although in such case power would suffer—i.e., the chances of committing a Type II error would increase since a slight inversion from the predicted order would necessitate rejection of the experimental hypothesis.

<sup>8</sup> Personal communication.

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## IDIODYNAMICS AND TRADITION

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By an *actual* historical accident the recent paper by Seeman and Galanter (4), attacking the writer's "Idiodynamics in Personality Theory" (2), appeared in the same issue with his "The Investigation of Repression as an Instance of Experimental Idiodynamics" (3). The latter article goes a long way toward a reply; only two or three additional comments are required.

1. Seeman and Galanter mistakenly assume that idiodynamics was considered to be wholly embodied in the projective methods; or, again, that the projective methods are a necessary part of the idiodynamic approach. The historical accidents which they cite are, indeed, accidental to the argument since idiodynamics, while implicit in and exemplified by the projective methods, extends far beyond them. The relevance of *experimental psychology* has been shown in the above-cited reference (2); the kinship to *psychotherapy* is found in the phenomenological approach now widely current. Under these circumstances the arguments presented by Seeman and Galanter which demonstrate that idiodynamics is not confined to the projective techniques are gratuitous.

If, as they also aver, the MMPI and other structured instruments are being developed along configurational lines, and the psychology of learning is, with the help of Skinner, recognizing the importance of emitted responses (response dominance), the conclusion to be drawn is that the orientation called *idiodynamic* is by no means limited to one aspect of psychology. It was for this very reason of breadth that the idiodynamic position was outlined in the

original paper as a theoretical basis for the empirically overdeveloped projective methods; but at no point was it there maintained that these techniques exhausted the theory or that it arose from them.

2. The critics' appeal to history demands scrutiny. It is to Skinner, they insist, that the revolution in respect to response dominance must be attributed. They write (4, p. 289): "We have shown that with respect to the 'postulate' of response dominance, the 'revolution in the conception of the stimulus' was instigated by Skinner rather than by psychodynamic or 'idiodynamic' psychologists." Here they are factually in error and the evidence is indicated in the paper they criticize. It was there shown that Thurstone in *The Nature of Intelligence* (6, Ch. I) and earlier (1923), in this very journal, cogently expounded his "stimulus-response fallacy." In these terms he takes to task the assumption that all behavior can be accounted for and controlled by the stimulus alone and forcibly calls attention to the important and academically neglected relationships between drive and response *within* the organism. In partial support of his position he quotes from Jennings as follows:

Activity does not require present external stimulation. A first and essential point for the understanding of behavior is that activity occurs in organisms without present specific external stimulation. The normal condition of *Paramecium* is an active one, with its cilia in rapid motion; it is only under special conditions that it can be brought partly to rest. *Vorticella*, as Hodge and Aikins showed, is at all times active, never resting. The same is true of

most other infusoria, and, in perhaps a less marked degree, of many other organisms. Even if external movements are suspended at times, internal activities continue. The *organism is activity*, and its activities may be spontaneous, so far as present external stimuli are concerned (1, p. 22).

But more saliently Thurstone makes explicit acknowledgment of his indebtedness to the "new psychology" of that time—psychoanalysis. Commenting, in summary, on this source of his ideas and on the vaunted objectivity of academic psychology, he writes (6, p. 164):

It is my belief that the attitude, which is implied in the so-called new psychology, has given, in a relatively short space of time, more insight into human conduct than the thoroughly objective point of view which has in recent years become established in scientific psychology, and which has been borrowed from related objective sciences.

Fourteen years later Skinner (5) presented the same fundamental idea as "the emitted response," revising the concept of the reflex and offering his contribution as an elaboration of behaviorism. (Incidentally, from the present ground of history it is a remarkable fact—perhaps a coincidence—that while Jennings' book, used by Thurstone, was entitled *Behavior of the Lower Organisms*, Skinner's later volume was called *Behavior of Organisms!*) Since Thurstone moved from the stimulus-response fallacy to factor analysis of the inner man while Skinner immediately exemplified his "operant behavior" in the field of animal learning, the thinking of the latter became more familiar than that of the former to some psychologists. But as *history* in modern psychology goes, not Skinner but Thurstone (and before him Jennings and Freud) must be credited with the formulation of what has been newly christened *response dominance*.

3. Seeman and Galanter make much of the threat which idiodynamics was said to present to traditional psychology, and their quotations are culled largely to support this interpretation. (This note has even crept projectively into their "fictitious structured personality test" [4, p. 287] in which the two "personologically significant" items of the three cited by them from a limitless, random universe involve rejection and distrust. Here the reaction of the authors will, no doubt, be: "Most notags karolize elatically"—which is their remaining item.) They then proceed to take up the cudgels for "tradition." But the idiodynamic orientation, which they characterize as belonging to clinical psychology as a whole, is by no means negatively motivated and is not inimical to all tradition. In fact, the just preceding references to Thurstone and Skinner—to say nothing of Freud—make it clear that some of psychology's traditions are being articulated in, not threatened by, idiodynamics. Psychology is and has been many things to many men and it can well afford to remain so. For where if not among psychologists should the idiodynamics of thought be exemplified!

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## KENDON SMITH'S COMMENTS ON "A NEW INTERPRETATION OF FIGURAL AFTER-EFFECTS"

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Comments, replies to comments, and often comments on replies to comments provide invigorating but seldom edifying contributions to our journals. They also take up space badly needed for new contributions. But since our first view of Dr. Smith's comments was on the pages of this journal (5), and since their tenor often implied misstatements on our part, I see no alternative but to make a public reply.

Smith justly criticizes us for failing to mention his critical note in the *American Journal of Psychology* on the satiation theory of the figural after-effect (4). Our only defense is that we were not attempting a complete review of the literature and did not come across his note in the other sources used. But let us look at the six points he raised there and repeated more recently:

1. *Neither satiation nor statistical theories explain after-effects like those obtained in the waterfall and Plateau spiral illusions.* These theories also do not explain contrast phenomena or the effect of values and motives upon perceived size—they are not required to cover all phenomena in the field of perception. Both of the illusions referred to involve continuous movement of contours in the field and may well involve mechanisms beyond area 17.

2. *Subjects who have worn distorting glasses for a long period show figural after-effects* (Gibson, 1). As I read Gibson (pp. 1-3), the effect of these prisms was such as to render vertical lines in the field curved (e.g., *I*-contours), and upon removal of the prisms truly vertical lines (e.g., *T*-contours) now appeared bent in the opposite di-

rection. Both Köhler and Wallach (2) and Heyer and myself (3), each in our own way, deal with this situation explicitly, specifying how displacements arise. I fail to see the point of the comment.

3. *Gibson (1) obtained figural after-effects "when his subjects merely viewed curved-line inspection-figures."* And Smith adds that "there was little if any opportunity for configurational adaptation" in this case. I assume that what Köhler and Wallach called "self-satiation" is referred to here, e.g., that certain figures may show form distortion during original inspection. The latter authors do go to considerable pains to interpret this phenomenon, pointing out that the effect would be expected for curved-line figures, where resistance is denser on one side than the other, but not for straight lines.

4. *Similar effects have been observed in other modalities.* This is exactly what one should expect if either satiation or statistical models apply to sensory projection systems in general.

5. *After-effects occur across the vertical meridian of the eye.* Heyer and myself went to considerable lengths to spell out this schism between functional and anatomical data.

6. *After-effects "in the third dimension" have been reported.* Rather than merely resolving this issue "in an expression of scepticism" as Smith says, we (a) explicitly discussed how "depth" and "size" interpretations can be shown to be interchangeable in many studies where cues are ambiguous and (b) described a duplication of the most criti-

cal demonstration of this effect with negative results.

Specifically against the statistical theory Smith offers two criticisms. (a) He says that "in Köhler and Wallach's basic demonstration, where the inspection-contours *are* congruent with the test contours, displacement of the latter is commonly reported" (5, p. 402) and he cites p. 271. This would be contrary to the satiation theory as well as the statistical theory. What do Köhler and Wallach (2) actually say on this matter? "The figure which coincides with the previously inspected object will be pale or gray in comparison with its black partner, it will seem to lie further back in space, and *it may look a trifle smaller*" (p. 271, italics mine). That *here* this is an "interpretation" based on the paleness cues, that *displacement* really does not occur under conditions of coincidence of *I* and *T* contours, is clearly indicated by Köhler and Wallach at many points. Witness the following: "But as we bring the *T*-line nearer and nearer, the satiated *I*-region will gradually extend *beyond* the place of the *T*-line. . . . This development will continue until the resistance on one side of the *T*-line is just as great as it is on the other side. *This happens in the position of coincidence where no displacement can occur*" (2, p. 338, italics mine). At no point do Köhler and Wallach report evidence contrary to this statement; Smith may have failed to take into account the careful distinction, made by both Köhler and Wallach and ourselves, between displacement and fading effects—both theories predict that the latter effect should be maximal at coincidence.

(b) It is true, as Smith claims, that the statistical theory could not explain

a movement of *T*-contours *toward* *I*-contours. But Köhler and Wallach do *not* mention this explicitly, as he further claims, citing p. 297; they describe a quite different situation. "If the *I*-object is an oblong, and if two *T*-squares are shown within the affected area, the distance *between these squares* is shortened" (2, pp. 296-7, italics mine). Note that it is the distance between the two *T*-squares, both retreating from opposite *I*-contours, that is decreased. A careful reading of the various examples in the following paragraph indicates that all the effects described are cases of displacement of *T*-contours away from *I*-contours—indeed, had this not been the case, Köhler and Wallach would have been obliged to count them as evidence contrary to their satiation theory. Smith concludes that he "has on file protocols of several rigorous observations by himself and by others in which this anomalous effect showed itself." Such observations would be of great theoretical significance and by all means should be reported in detail.

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## THE LINEAR OPERATOR OF BUSH AND MOSTELLER

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The purpose of this paper is to correct a semantic misunderstanding about the two parameters in the linear operator of Bush and Mosteller (2). Their basic equation was written as

$$Qp = p + a(1 - p) - bp,$$

where  $a$  and  $b$  are parameters,  $p$  is the probability of response in a specified interval of time, and  $Qp$  is the probability of response in the next interval of time. The statement of Bush and Mosteller, recently criticized by this writer (1), read as follows: "To maintain the probability between 0 and 1, the parameters  $a$  and  $b$  must also lie between 0 and 1" (2, p. 315).

It is likely that some readers interpreted this sentence correctly. Its lack of complete exactness, however, led this writer (1) to misunderstand Bush and Mosteller. The misinterpretation was this: If  $a$  and  $b$  are chosen outside of the open interval  $(0, 1)$ , then for *no* value of  $p$  in the closed interval is it possible to calculate a value of  $Qp$  in the open interval. Taken as it stands, this statement is clearly false, as the writer demonstrated (1). Bush and Mosteller, however, did not mean this.<sup>1</sup> What they did mean is the following: A necessary condition for the proper restriction of  $Qp$  ( $0 \leq Qp \leq 1$ ) for *all* admissible values of  $p$  ( $0 \leq p \leq 1$ ) is that *both*  $a$  and  $b$  are restricted ( $0 \leq a \leq 1$ ,  $0 \leq b \leq 1$ ). The proof follows from the basic equation, here written as

$$Qp = a(1 - p) + (1 - b)p.$$

<sup>1</sup> Bush and Mosteller. Personal communication. 1952.

Since this is assumed for all admissible values of  $p$ , set  $p = 0$ , whence  $0 \leq a \leq 1$ ; set  $p = 1$ , whence  $0 \leq 1 - b \leq 1$ , and thus  $0 \leq b \leq 1$ .

The corresponding sufficiency theorem may now be stated: A sufficient condition for the proper restriction of  $Qp$  ( $0 \leq Qp \leq 1$ ) is the restriction of  $p$ ,  $a$ , and  $b$  ( $0 \leq p \leq 1$ ,  $0 \leq a \leq 1$ ,  $0 \leq b \leq 1$ ). A derivation based upon a closely related proof by R. D. Luce<sup>2</sup> is as follows: Since  $0 \leq a \leq 1$ , and  $0 \leq 1 - p \leq 1$ , therefore  $0 \leq a(1 - p) \leq 1 - p$ . Since  $0 \leq 1 - b \leq 1$ , and  $0 \leq p \leq 1$ , therefore  $0 \leq (1 - b)p \leq p$ . Therefore, by addition,  $0 \leq Qp \leq 1$ .

The reader should note that the necessity theorem is not the converse of the sufficiency theorem, since the restriction of  $p$  to the closed interval  $(0, 1)$  is postulated, not derived, in both. This lack of a converse relationship between the two theorems was a contributing cause to the writer's misunderstanding.

If  $0 \leq a \leq 1$  and  $0 \leq b \leq 1$ , then  $-1 \leq 1 - a - b \leq 1$ . When Bush and Mosteller applied their basic theory to certain problems in operant conditioning, however, they were implicitly requiring more stringent restrictions on  $a$  and  $b$ .

In the first place, there is the restriction that  $0 \leq a + b \leq 1$ . Consider their Equation 4 (2, p. 316) which gives  $Q^n p$  as a function of  $n$ . It is not necessary for this function to be continuous in  $n$ . But later on their use of the calculus does imply this continuity. Therefore, whenever the calculus is used,  $1 - a - b$  is nonnegative.

<sup>2</sup> R. D. Luce. Personal communication. 1952.

Since this quantity is already restricted to the closed interval  $(-1, 1)$ , therefore  $0 \leq 1 - a - b \leq 1$ , and thus  $0 \leq a + b \leq 1$ .

In the second place, the calculus implies that both  $a$  and  $b$  (and their sum) must be very small compared to 1. Bush and Mosteller (2, p. 317) state that

$$\Delta p = a(1 - p) - bp.$$

Since the calculus implies continuity in  $n$ , therefore, as  $\Delta n$  approaches zero so does  $\Delta p$ . Since this is true for all permissible values of  $p$ , set  $p = 0$ , whence  $a$  approaches zero. Set  $p = 1$ , whence  $-b$  (and thus  $b$ ) approaches zero; and consequently so does  $a + b$ .

The values of  $a$  and  $b$  obtained to date from curve fitting are quite consistent with these conclusions. Bush and Mosteller computed the following values:  $a = 0.014$ ,  $b = 0.026$  (2, p. 320).

The reader should note that so far we have been concerned almost entirely with mathematical matters. What we have deduced is logically true, whether or not the theory is empirically true. Can we, however, rule out in advance the possibility of finding values of the parameters  $a$  and  $b$  outside the closed interval  $(0, 1)$ ? The writer believes that we should not dismiss the possibility. Bush and Mosteller (2, p. 314) derived the linear operator as a "first approximation." It is possible, however, that a more

adequate expression for the operator may be nonlinear. If so, a linear approximation may still be quite adequate over a subinterval of the admissible range of  $p$ , but not over the entire closed interval  $(0, 1)$ . In this case, the restrictions on  $a$  and  $b$  need not hold at all, and yet  $Qp$  may still be properly restricted. We have seen that the values of  $a$  and  $b$  reported to date by Bush and Mosteller are quite properly restricted and so is their sum. If, however, empirical values for  $a$  and  $b$  are ever obtained outside their proper range, then *one* linear operator will not be an adequate approximation over the entire admissible range of  $p$  (for that research at least).

Unless experimental evidence forces us to abandon the linear operator, its simplicity is a strong argument for its continued use. As long as the linear operator is used, especially in conjunction with the calculus, the proper restrictions upon the ranges of the two parameters will be basic to any extension of the theory. That is the justification for the detailed study of this problem.

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## ON A DEFINITION OF CULTURE

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In *Learning Theory and Culture* (1, p. 385) a definition of culture is developed and presented as, "'Culture' = 'ŷ (x learns y from z and  $x \neq z$ )' by df. (to be read 'the class of values of the variable y such that x learns y from z, and x is not identical with z')." This definition is arrived at by examining seven fundamental assumptions of Murdock (2) and discarding four of them while retaining two.<sup>1</sup>

From their text we learn that culture thus defined can be read as: culture is defined as the class of responses of any hominid individual learned from any other hominid individual. Culture thus defined appears to be an area of investigation for psychologists (as scientists who are interested in studying the behavior of individuals as such) rather than anthropologists or sociologists whose interest in studying behavior is focused more upon the behavior of groups. Furthermore, the process itself ( $x$  learns  $y$  from  $z$  and  $x \neq z$ ) by which culture ( $\hat{y}$ ) is mediated is the area of psychological investigation which goes by the general name of learning theory and more specifically by the name of human learning.

A stated reason for Moore and Lewis' interest in framing a definition of culture is that it should "... have maximum utility in facilitating cross-disciplinary cooperation between psychology, anthropology, and sociology" (1, p.

384). However, by their definition they have cast out any utility in involving anthropologists and sociologists and instead have implicitly indicated a need for cooperation between psychologists and educators whose main job, it may be said, is to facilitate the process whereby ( $x$  learns  $y$  from  $z$  and  $x \neq z$ ).

In order to bring the anthropologists and sociologists back into the picture it seems necessary to involve, in some way, the concept of the "group" as part of the definition. This may be done as follows: "culture =  $\hat{y}$  (x learns y from z and  $x \neq z$ ) and where x and z are both members of some homogeneous group." Homogeneous group may be defined as a collection of individuals ( $x_1, x_2, \dots, x_{n-1}, x_n$ ) who already share certain common characteristics. These "common characteristics" may be specifically delimited in anthropological and sociological terms as indicated by findings from research in group membership.

Only by paying some respect to the "group nature" of culture can the anthropologists and sociologists be included in cross-disciplinary cooperation. That they should be included is certainly indicated by dint of their previous work in the field.

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## THE CIRCUMNAVIGATION OF COGNITION

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Columbus Day is an occasion in this country for celebrating the belief that the earth is round. Since 1492 this idea has caught on so well that it is now a part of the public school curriculum. Opposition to it has virtually disappeared. Today, however, certain new ideas in modern science suggest that we may have been too hasty in our judgment and that this belief may be quite misleading if not actually false. Now by "modern science" I do not mean what you think. I mean, instead, the new methods of "theory construction" as they are called, devised by psychologists.

These methods were devised, of course, to deal with specific psychological problems, but their use need not and indeed should not be limited to these problems. To my knowledge the present paper is the first to apply these methods to problems outside the social sciences. The problem we have chosen is the problem of the earth's shape. Is it round or is it flat? The analysis we have chosen is one recently used by Kendler (4) in his discussion of a similar problem in psychology.

### WHAT IS THE SHAPE OF THE EARTH?

Geographers have disputed about the shape of the earth since Pythagoras first suggested that it was round rather than flat. This is certainly not the place to review all the arguments, but there is one argument which we must discuss. I refer to the argument based upon what is called "the phenomenon of circumnavigation." By this the ball theorists, as they are called, mean that explorers who set out from some place and keep sailing in a constant direction, eventu-

ally return to the place from whence they started. The results of the explorations of Magellan (1), Drake (3) and Captain Cook (8) are all illustrations of this phenomenon. The ball theorists claim that these results contradict the basic assumptions of the disk theory, and they surely seem to, at first sight. But before we decide let us consider the replies which the disk theorists have made to this argument.

Some disk theorists (5) reply by demonstrating that, no matter what the facts appear to be, circumnavigation is impossible. This demonstration is based upon an analysis of the word "to navigate." How do we know, say these theorists, when navigation has occurred? We can only know this if the navigator has moved from one place to another, in short, the empirical meaning of the word means to go to *another* place. Thus the very notion of "circumnavigation" is contradictory since it means to navigate to one's starting place. In this sense the phenomenon is impossible.

Other disk theorists (7) admit that circumnavigation is possible, but seriously doubt that it ever occurs. The fact that a few explorers have "circumnavigated," they say, is given all too much importance. Consider instead the many, many, explorers who have set out to circumnavigate and have failed. Thus the few cases of so-called "circumnavigation," they say, might easily be expected simply on the basis of chance.

There are other disk theorists (6) who admit that circumnavigation does occur, but think that it is a mighty poor way to travel. They point, for example, to the great numbers of travelers who have successfully returned home

by retracing their original route. Thus they demonstrate that the chances of safe return are much greater by this method than by the method of circumnavigation.

The issue, say other disk theorists (2, 9), is not a theoretical one at all. Of course, "circumnavigation" in *some* sense occurs. But in *what* sense is the crucial question. Only when we have discovered all the factors that produce circumnavigation, will we be able to answer this question. And when we have done this, there will be no issue left for theoretical dispute. The facts will have provided the answer.

Finally there are those who might be called "the semantical disk theorists." They say that the controversy results from the use of words. Its solution consists in recognizing that what ball theorists mean by the word "round" is what everyone else means by the word "flat." Once the appropriate word substitutions are made, the problem is resolved. There also is a group of "semantical ball theorists" who apply the same kind of analysis to the problem. They conclude that what the disk theorists mean by "flat" is what everyone else means by "round." The only problem that remains is to decide which "semantical analysis" is the correct one.

So much for the present status of the controversy concerning the shape of the earth. Can the controversy be settled? Certainly there is little hope of either side giving in. What, then, is to be done? Perhaps it is time to apply methodological analysis. Kendler (4) reported great success following his use of such an analysis. He applied it to a problem concerning the nature of learning about which there had been a long and apparently irreconcilable controversy. Following a single application of methodological analysis the controversy was resolved. Because of the remarkable success of this approach to

that problem we shall employ the same analysis to the problem of the earth's shape.

#### THE QUESTION AND ITS ANALYSIS

Present-day philosophy of science has devised criteria for discriminating questions that are meaningless from those that are not. So, whenever a question is posed that no one is able to answer, it is time to ask whether the question is answerable. "By application of methodological analysis," says Kendler (4, p. 269), "it is possible to demonstrate that certain problems are not resolvable, not because they are too profound, but rather because the questions they raise cannot be properly answered."<sup>1</sup> If the question can be shown to be a pseudo-question, then all sensible persons will refrain from asking it, and our inquiry can be directed to more fruitful problems. It is the purpose of the present analysis to show that the question, "What is the earth's shape?" is such a pseudo-question, and so should not be asked.

Now of course most geographers not only regard this as a sensible question, but also believe that an answer to it is crucial to an understanding of geography. On the other hand, as we have seen, empirical evidence refuses to provide us with an answer. Consider, for example, the results from various balloon ascensions made by geographers seeking an answer to this question. When they came down and described what they saw from aloft, the descriptions of the ball theorist and the disk theorist were alike in every detail. There is only one difference between them. One describes the earth's surface as round, the other as flat. How is this possible? Methodological analysis states that such a paradox arises whenever the question posed is a

<sup>1</sup> Unless specifically noted, all further quotations will be from Kendler's paper (4).

pseudo-question. This is expressed in one of the fundamental principles of methodological analysis.

If comparable data are employed to support diverse answers to the same question, then the major source of difficulty lies not in the seemingly opposed answers but, rather, in the question itself.

Now how do these conflicting notions about the shape of the earth arise? When we read the theoretical papers of various geographers, these notions appear to be basic to the theories presented. But are they really? Now, no matter how convinced a geographer may be that his notions about the earth's shape are essential to his *thinking*, these notions may be quite external to his *theory*. At this point it may be helpful to introduce another principle of modern methodology. According to this second principle it is essential to distinguish between a scientist's *thinking* and his *theory*. It was formerly believed that a scientist's thinking produced his theory, and as a result his theory represented his thinking. But this is all wrong. It is based upon a prescientific notion of causation, and so is rejected by modern methodology. In its place we have the sharp distinction between thinking and theory. So, although a geographer may think a great deal about the shape of the earth, his theory need not and perhaps should not make any reference to the earth's shape. At this point the reader may find this distinction between thinking and theory puzzling. However, when we see what is considered "theory" by modern methodology, the distinction should become obvious. And to this matter we now turn.

It has been suggested that notions about the earth's shape may be external to geographical theory. To decide this question, we must review what are called

"the structural requirements" of a geographical theory.

The geographer is concerned with stating in as precise a way as he can where things are. This task has two aspects: (a) the stating of the location of some given thing or group of things, and (b) the description of the thing or group of things in a given location. Now in order to do this the geographer must travel from place to place noting first what is in each place and second how he got to each place. Thus his empirical "first-order laws" as they are called take the following form:

If I start from place A, and go a certain distance in such and such direction, then I will reach B.

Such a first-order empirical law describes the relation between the independent variables of starting place, direction, and distance, and the dependent variable of terminal place, which results when the antecedent conditions specified by the independent variables, are satisfied. So far, the geographer has no need of theory. If he wishes, he can merely make a list of all the empirical laws discovered in his travels and do without theory. But the geographer, if he travels enough, will discover two remarkable things which may lead him to begin theorizing.

First, he will discover that place B can be reached from a variety of starting places. If he is in a methodological mood, he may express this by saying that the dependent variable is a function of several independent variables. Secondly, he is likely to observe that different terminal places can be reached from the same starting place. These two discoveries lead the geographer to construct or erect a theory. He does this by making a map on which place B, as well as many other places, is represented. The map shows how it is possible to get to B from many of these

places, and also how it is possible to get to many of these places from place B. Such a map, speaking methodologically of course, "bridges the gap existing between the independent and dependent variables."

The geographer with a methodological orientation prefers such maps, which he calls "intervening variables," to a list of directions or rules for getting from one place to another. As he puts it, he would rather create such a theory than "treat separately the relationship each independent variable bears to many dependent variables," and vice versa. It is, of course, important to understand that the intervening variable is not discovered by the geographer. Not at all. It is invented or constructed by him and "this intellectual construction," as he will tell you, "has as its aim the economical description of the known empirical relationships and the prediction of new phenomena." Once you have grasped these essentials of theory construction you are in a position to understand "the structural requirements" of a geographical theory. Such a map, or theoretical erection, must, if it is not to collapse, be anchored to the antecedent independent variables on the one side, and to the consequent dependent variables on the other. Any map which is not so anchored is useless for guiding us anywhere, and so is said to be without operational meaning. Thus, the structural requirements of a theory state in a very methodological way the conditions which ensure that the theory has operational meaning.

It is clear from all this that the structural requirements of a geographical theory include no references to the shape of the earth. In this sense, at least, such statements are external to theory. But modern methodology reveals an even deeper sense in which this is true. Consider for a moment the first-order empirical laws of a ball and a disk

theorist. Will there be any differences in these laws? No, for the consequences of going a certain distance in a certain direction from a given starting place will be the same for both. Since a map is merely a shorthand description of such empirical laws there can be no *operational* differences between the maps of two "opposed" theorists. Thus in a deeper methodological sense statements about the shape of the earth are external to geographical theory.

But what, the reader may ask, am I talking about when I say that the earth is not flat? The methodological answer to this question is simple and direct. Nothing! Such statements, the methodologist will tell you, "represent secondary and unnecessary elaborations about the meaning of these intervening variables." You would never make such completely pseudo-statements if you remembered that these intervening variables serve as economical devices to "order" the relations expressed in our first-order laws. These maps, he will go on, "are *shorthand descriptions* and nothing more. . . . The only *meaning* possessed by these intervening variables is their relationship to both the independent and dependent variables. Because this point has been ignored, an immense amount of confusion concerning the 'real meaning' of these intervening variables exists."

But why, one may ask, has such an obvious point been so persistently ignored? The reason, says modern methodology, is the "fallacy of reification or hypostatization." This fallacy consists in regarding certain words as names of things or entities when they aren't. Let's begin by assuming we know what is meant by the words "thing" and "entity." Without such an assumption it is very difficult to make this fallacy understood.

Any *thing* can be given a proper name like "Julius Caesar" or "53A270," and

can also be given a class name like "Roman general" or "Ford sedan." Now, although all class names in English are nouns, not all English nouns are class names. This is most easily illustrated with slang expressions like: "He threw a tantrum," or "She copped a gander." In such cases it is clearly silly to ask where the tantrum is that was thrown, or the gander that was copped. The reason why it is silly is that the nouns "tantrum" and "gander" have no meaning apart from these phrases in which they appear. The fallacy of reification is committed when you regard such a noun as a class name referring to things or entities.

Now as we have seen, a geographer's duties consist in traveling, recording observations on a map. This whole complex process is called "mapping the earth." The geographer commits the fallacy when he thinks of the word "earth" as having some meaning apart from this phrase. He then regards the word "earth" as a class name and imagines that it refers to some thing or entity. It is thus, modern methodology makes clear, that the fallacy of reification creates the problem of the earth's shape. The realization that the word "earth" does not refer to a thing or entity, disposes of the problem.

#### THE USE AND ABUSE OF INTUITIVE MODELS

Hume recommended that nonsense, when discovered, be committed to the flames. In his view it could contain "nothing but sophistry and illusion." Modern methodology, however, is not so reckless with the products of human creation. Although, as we have seen, statements about the shape of the earth are nonsense, we should not conclude from this that such statements are worthless. Far from it. They serve to help the geographer in his construction of what is called an "intuitive model."

This model serves as a "*thinking aid*" leading to the invention of theoretical constructs and intervening variables. Some geographers, for reasons which are not yet fully understood, get more help from thinking of the earth as flat, others are helped more by thinking of it as round. "It would be hazardous, as well as somewhat presumptuous, for any theorist to insist that every theorist think in his style."

The failure of Hume and others to recognize the usefulness of such nonsense was due to their misunderstanding of the relation between thinking and theory. As we have pointed out, modern methodology makes a sharp distinction between "the personal thought processes leading to the invention of theoretical constructs and the operational meanings" of these constructs.

But the fact that such meaningless statements form the core of scientific thinking should not mislead the reader into thinking that such statements are capable of being either true or false. Modern methodology insists that the decision between various such intuitive models "is in the last analysis a decision having no *truth character*. That is, in spite of the fact that the choice of a model may, and usually does, influence both experimentation and theorizing, the *choice itself* cannot be evaluated as being right or wrong. It is a matter purely of personal taste. The most we can do is to attempt, in a sincere and conscientious manner, to understand the implications of such decisions, but we should not be led astray by believing we can experimentally test their validity. . . ."

#### SUMMARY

We have almost completed the "circumnavigation of cognition." One further methodological homily will serve to end the trip. Henry Fielding in *Tom Jones* has this to say:

The only supernatural agents which can in any manner be allowed to us moderns, are ghosts; but of these I would advise an author to be extremely sparing. These are indeed, like arsenic, and other dangerous drugs in physic, to be used with the utmost caution; nor would I advise the introduction of them at all in those works, or by those authors, to which, or to whom, a horse-laugh in the reader would be any great prejudice or mortification.

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